

AJS

85

American
Journal
of Sociology

Volume 95 Number 1
July 1989

Media Discourse and Public Opinion on Nuclear Power—
Gamson and Modigliani

Structure after 50 Years—Camic

Fathers' Ages and the Social Stratification of Sons—
Mare and Tzeng

Self-Perceptions of Black Americans—Hughes and Demo

Occupational Segregation by Sex—Jacobs

The University of Chicago Press

WILLIAM L. PARISH, Editor

MARY C. BRINTON, PETER HEDSTROM, and FRED KNISS, Associate Editors

WENDY GRISWOLD and BRUCE G. CARRUTHERS, Book Review Editors

VIRGINIA HARRIS BARTOT, DANIEL BRESLAU, LINGXIN HAO, SUN-HWA LEE, and MARTHA VAN HAITSMA, Associate Book Review Editors

MARK JACOBS, Editorial Manager

JANE MATHER, Editorial Assistant

Consulting Editors ELIJAH ANDERSON · RICHARD T. CAMPBELL · MARY L. FENNELL · NEIL FLIGSTEIN · DAVID F. GREENBERG · CHARLES N. HALABY · CAROL A. HEIMER · LARRY W. ISAAC · ROBERT ALUN JONES · LESTER R. KURTZ · ROSS L. MATSUEDA · ANN S. ORLOFF · TROND PETERSEN · WALTER W. POWELL · JONATHAN RIEDER · RACHEL A. ROSENFIELD · MICHAEL E. SOBEL · AAGE B. SØRENSEN · MARK C. STAFFORD · RONNIE STEINBERG · JOHN D. STEPHENS · IVAN SZELENYI · EDWARD A. TIRYAKIAN · MARK TRAUGOTT · GAYE TUCHMAN · LINDA J. WAITE · CAROL A. B. WARREN · SUSAN COTTS WATKINS · DAVID ZARET

International Consulting Editors HANS-PETER BLOSSFELD (West Germany) · RAYMOND BOUDON (France) · PIERRE BOURDIEU (France) · MONICA BOYD (Canada) · MARTIN BULMER (England) · FRANCIS G. CASTLES (Australia) · BONNIE H. ERICKSON (Canada) · BERT KLANDERMANS (The Netherlands) · BRYAN S. TURNER (The Netherlands)

Editorial Board C. ARNOLD ANDERSON · GARY S. BECKER · RICHARD G. BIERNACKI · CHARLES E. BIDWELL · DONALD J. BOGUE · TERRY NICHOLS CLARK · JAMES S. COLEMAN · JOHN L. COMAROFF · LEO A. GOODMAN · PHILIP M. HAUSER · EVELYN KITAGAWA · NANCY LANDALE · EDWARD O. LAUMANN · DONALD N. LEVINE · DOUGLAS S. MASSEY · JOHN F. PADGETT · EDWARD SHILS · GEORGE STEINMETZ · FRED L. STRODTBECK · GERALD D. SUTTLES · MARTA TIENDA · WILLIAM J. WILSON

THE AMERICAN JOURNAL OF SOCIOLOGY (ISSN 0002-9602) is published bimonthly in July, September, November, January, March, and May by The University of Chicago at The University of Chicago Press, Journals Division, P.O. Box 37005, Chicago, Illinois 60637. Subscription rates, U.S.A.: institutions, 1 year \$64.00; individuals, 1 year \$32.00. Student subscription rate, U.S.A.: 1 year \$23.00 (photocopy of valid student ID must accompany subscription). ASA and BSA members, 1 year \$27.00. All other countries add \$8.00 for each year's subscription to cover postage. Subscription agent for Japan: Kinokuniya Company, Ltd. Single copy rates: institutions \$11.00, individuals \$5.50. Back issues available from 1982 (vol. 88). Volumes in microfilm available from University Microfilms International, 300 North Zeeb Road, Ann Arbor, Michigan 48106; and Kraus Reprint and Periodicals, Route 100, Millwood, New York 10546. Subscriptions are payable in advance. Subscriptions will be entered to start with the first issue to be published after receipt of order. *Claims for missing numbers* should be made within the month following the regular month of publication. The publisher expects to supply missing numbers free only when losses have been sustained in transit and when the reserve stock will permit. *Postmaster:* Send address changes to The University of Chicago Press, Journals Division, P.O. Box 37005, Chicago, Illinois 60637.

Copying beyond Fair Use. The code on the first page of an article in this journal indicates the copyright owner's consent that copies of the article may be made beyond those permitted by Section 107 or 108 of the U.S. Copyright Law provided that copies are made only for personal or internal use or for the personal or internal use of specific clients and provided that the copier pay the stated per-copy fee through the Copyright Clearance Center, Inc. Operations Center, 27 Congress St., Salem, Massachusetts 01970. To request permission for other kinds of copying, such as copying for general distribution, for advertising or promotional purposes, for creating new collective works, or for resale, kindly write to Permissions Department, The University of Chicago Press, 5801 S. Ellis Ave., Chicago, Illinois 60637. If no code appears on the first page of an article, permission to reprint may be obtained only from the author. Second-class postage paid at Chicago, Illinois, and at additional mailing offices.

© 1989 by The University of Chicago. All rights reserved.

The paper used in this publication meets the minimum requirements of American National Standard for Information Sciences—Permanence of Paper for Printed Library Materials, ANSI Z39.48-1984. ☠

CULC-H02839-85-P005002

American Journal of Sociology

AJS

Volume 95 No. 1 July 1989

CONTENTS

1 Media Discourse and Public Opinion on Nuclear Power: A Constructionist Approach
WILLIAM A. GAMSON AND ANDRE MODIGLIANI

38 Structure after 50 Years: The Anatomy of a Charter
CHARLES CAMIC

108 Fathers' Ages and the Social Stratification of Sons
ROBERT D. MARE AND MEEI-SHENN TZENG

132 Self-Perceptions of Black Americans: Self-Esteem and Personal Efficacy
MICHAEL HUGHES AND DAVID H. DEMO

160 Long-Term Trends in Occupational Segregation by Sex
JERRY A. JACOBS

85

85

Commentary and Debate

174 Region and Violent Attitudes Reconsidered: Comment on Dixon and Lizotte
CHRISTOPHER G. ELLISON AND PATRICIA L. McCALL

178 On Cultural Explanations of Southern Homicide: Comment on Dixon and Lizotte
JAY CORZINE AND LIN HUFF-CORZINE

182 The Burden of Proof: Southern Subculture-of-Violence Explanations of Gun Ownership and Homicide
JO DIXON AND ALAN J. LIZOTTE

188 Comment on Varese's Review of *Middle American Individualism: The Future of Liberal Democracy*
HERBERT J. GANS



Book Reviews

191 *American Families and Households* by James A. Sweet and Larry L. Bumpass
ARLAND THORNTON

193 *Society and Family Strategy: Erie County, New York 1850–1920*
by Mark J. Stern
KATHRYN M. NECKERMAN

194 *Women Adrift: Independent Wage Earners in Chicago, 1880–1930*
by Joanne J. Meyerowitz
LISA M. FINE

196 *Putting on Appearances: Gender and Advertising* by Diane Barthel
ELLA TAYLOR

198 *Women of Academe: Outsiders in the Sacred Grove* by Nadya Aisenberg and Mona Harrington
MARY JO DEEGAN

200 *First Births in America: Changes in the Timing of Parenthood* by Ronald R. Rindfuss, S. Philip Morgan, and Gray Swicegood
BETH OSBORNE DAPONTE

202 *The American Census: A Social History* by Margo J. Anderson
WILLIAM PETERSEN

203 *Migration and Politics* by Thad A. Brown
RODOLFO O. DE LA GARZA

205 *Children of Circumstances: Israeli Emigrants in New York* by Moshe Shokeid
WILLIAM B. HELMREICH

207 *Familiar Strangers: Gypsy Life in America* by Marlene Sway
BEVERLY Y. NAGEL

209 *Hewers of Wood and Drawers of Water: Noncitizen Arabs in the Israeli Labor Market* by M. Semyonov and Noah Lewin-Epstein
SASKIA SASSEN

210 *Work and Industry: Structures, Markets, and Processes* by Arne L. Kalleberg and Ivar Berg
RANDY HODSON

212 *The Mobility of Capital and Labor: A Study in International Investment and Labor Flow* by Saskia Sassen
JOHN WALTON

215 *Falling from Grace: The Experience of Downward Mobility in the American Middle Class* by Katherine S. Newman
STEPHEN J. MCNAMEE

217 *City, State, and Market: The Political Economy of Urban Society* by Michael Peter Smith
JOHN R. LOGAN

218 *Socialist Entrepreneurs: Embourgeoisement in Rural Hungary* by Ivan Szelenyi, in collaboration with Robert Manchin, Pál Juhász, Bálint Magyar, and Bill Martin
STEVEN BRINT

220 *Of Rule and Revenue* by Margaret Levi
JOHN MARKOFF

222 *The Social Origins of Political Regionalism: France, 1849–1981* by William Brustein
JOHN MARKOFF

224 *Constitution Making: Conflict and Consensus in the Federal Convention of 1787* by Calvin C. Jillson
SUZETTE HEMBERGER

226 *Fiscal Limits of Absolutism: Taxation in Early Seventeenth-Century France* by James B. Collins
MARGARET LEVI

228 *Sectarian Violence: The Liverpool Experience, 1819–1914* by Frank Neal
TONY LANE

229 *The Road to Mobocracy: Popular Disorder in New York City, 1763–1834* by Paul A. Gilje
TED ROBERT GURR

231 *Policing the Poor in Eighteenth-Century France* by Robert M. Schwartz
HILTON L. ROOT

232 *Console and Classify: The French Psychiatric Profession in the Nineteenth Century* by Jan Goldstein
ROBERT ALUN JONES

234 *Autocracy, Capitalism, and Revolution in Russia* by Tim McDaniel
JEFFREY BROOKS

236 *Cultural Revolution in China's Schools, May 1966–April 1969* by Julia Kwong
THOMAS B. GOLD

238 *Rationality and Revolution* edited by M. Taylor
WILLIAM BRUSTEIN

239 *The Organizational State: Social Choice in National Policy Domains* by Edward O. Laumann and David Knoke
CHARLES TILLY

242 *Reconstructing American Education* by Michael B. Katz
KATHLEEN BARRY

243 *Community Organizations: Studies in Resource Mobilization and Exchange* by Carl Milofsky
DAVID KNOKE

245 *Structural Equation Modeling with LISREL: Essentials and Advances* by Leslie A. Hayduk
D. RANDALL SMITH

247 *Analysing Everyday Explanation: A Casebook of Methods* edited by Charles Antaki
KATHY CHARMAZ

248 *Under Cover: Police Surveillance in America* by Gary T. Marx
LYNN ZIMMER

250 *The Social Psychology of Procedural Justice* by E. Allan Lind and Tom R. Tyler
HENRY A. WALKER

252 *Juvenile Correctional Reform: Two Decades of Policy and Procedural Change* by Edmund F. McGarrell
MARK D. JACOBS

254 *Social Epistemology* by Steve Fuller
STEVEN YEARLEY

256 *Explaining Science: A Cognitive Approach* by Ronald N. Giere
ADELE E. CLARKE

258 *Narrative Knowing and the Human Sciences* by Donald E. Polkinghorne
ROBERT J. RICHARDS

260 *Feminist Theory and the Philosophies of Man* by Andrea Nye
SONDRA FARGANIS

262 *Max Weber: Essays in Reconstruction* by Wilhelm Hennis. Translated by Keith Tribe
BRAULIO MUÑOZ

263 *The Causes of Progress: Culture, Authority and Change* by Emmanuel Todd. Translated by Richard Boulind
HARVEY J. GRAFF

265 *War Memorials as Political Landscape: The American Experience and Beyond* by James M. Mayo
BARRY SCHWARTZ

268 *Freak Show: Presenting Human Oddities for Amusement and Profit* by Robert Bogdan
STEVEN C. DUBIN

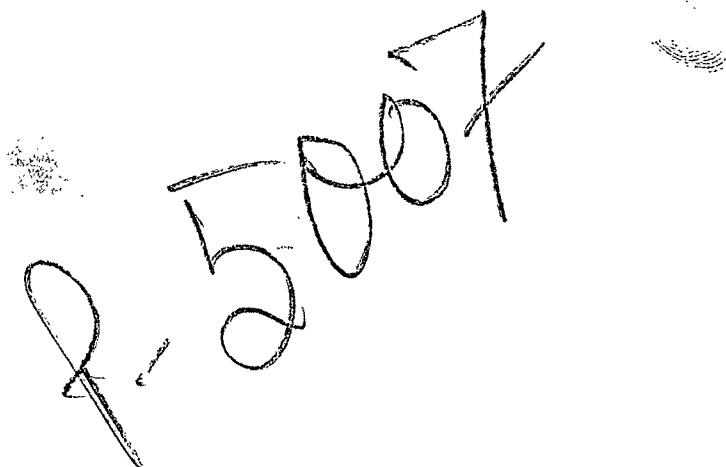
270 *The Culture of Capital: Art, Power and the Nineteenth-Century Middle Class* editec by Janet Wolff and John Seed
DAVID BRAIN

272 *Culture and Consumption: New Approaches to the Symbolic Character of Consumer Goods and Activities* by Grant McCracken
WILLIAM LEISS

273 *Becoming an EX: The Process of Role Exit* by Helen Rose Fuchs Ebaugh
LUCINDA F. SAN GIOVANNI

275 *New Wine in Old Wineskins: Evangelicals and Liberals in a Small-Town Church* by R. Stephen Warner
NANCY T. AMMERMAN

277 *Tales of the Field: On Writing Ethnography* by John Van Maanen
DENNIS RAY WHEATON

A handwritten signature in black ink, reading "John Van Maanen". The signature is written in a fluid, cursive style, with "John" on the left, "Van" in the middle, and "Maanen" on the right. The "a" in "Van" and the "a" in "Maanen" are capitalized and have small loops. There are some small, illegible marks or signatures to the right of the main name.

IN THIS ISSUE

WILLIAM A. GAMSON is professor of sociology at Boston College. He is writing a book on how people think and talk about political issues and the ways in which they use media discourse and the larger political culture it reflects. He directs the Media Research and Action Project at Boston College, a group focused on the media and public education strategies of social change organizations.

ANDRE MODIGLIANI is a social psychologist and associate professor of sociology at the University of Michigan, Ann Arbor. His interests are understanding public opinion on political issues as an interaction between individual cognitive styles and the manner in which these issues are conceptualized in the mass media, and understanding differences and similarities in the motivation for, and social control of, crimes of the powerless (e.g., robbery and burglary) versus crimes of the powerful (e.g., corporate crime and rape).

CHARLES CAMIC is professor of sociology and associate chairman of the Department of Sociology at the University of Wisconsin—Madison. His interests are theory, historical sociology, and the sociology of knowledge. He is editor of *The Early Essays of Talcott Parsons* (forthcoming).

ROBERT D. MARE is professor of sociology at the University of Wisconsin—Madison. His research focuses on links between labor markets and marriage markets, processes of educational stratification, statistical models for couples' behavior, and models for categorical data.

MEEI-SHENN TZENG is a graduate student at the University of Wisconsin—Madison. She is working on social stratification and marriage markets in the United States.

MICHAEL HUGHES is assistant professor of sociology at Virginia Polytechnic Institute and State University. Most of his work has focused on microenvironmental determinants of mental health and well-being. Recently he has examined issues concerning psychological well-being, color, and ethnic identity among black Americans. He is finishing a monograph on living alone.

DAVID H. DEMO is assistant professor of sociology at Virginia Polytechnic Institute and State University. His interests are the linkages between social structure and personality, especially the social correlates of self-concept and the influence of family relations on children.

JERRY A. JACOBS is an assistant professor of sociology at the University of Pennsylvania. He has recently published *Revolving Doors: Sex Segregation and Women's Careers* (Stanford University Press, 1988). His current projects include a study of internal labor markets and a study of physicians' career patterns.

Information for Contributors

Editorial Procedures: All papers deemed appropriate for the *AJS* are sent out anonymously to readers. The editors rely heavily on the judgments of those readers, although they are not bound by them. To protect anonymity, only the title should appear on the manuscript. Attach a cover page with the title of the manuscript, your name, and your affiliation. All identifying references and footnotes should appear on a separate page. A legible, carefully prepared manuscript will facilitate the work of readers. Manuscripts are accepted for publication subject to nonsubstantive editing with the understanding that the *AJS* has the right of first publication. Submission of a manuscript to the *AJS* is taken to indicate the author's commitment to publish in this *Journal*. No paper known to be under consideration by any other journal will be reviewed by the *AJS*. Upon publication, all rights, including subsidiary rights, are owned by the University of Chicago Press. The author retains the right to use his article without charge in any book of which he is the author or editor after it has appeared in the *Journal*.

Manuscript Acceptance Policy: While it is our policy to require the assignment of copyright on most journal articles and review essays, we do not usually request assignment of copyright for other contributions. Although the copyright to such a contribution may remain with the author, it is understood that, in return for publication, the journal has the nonexclusive right to publish the contribution and the continuing right, without limit, to include the contribution as part of any reprinting of the issue and/or volume of the journal in which the contribution first appeared by any means and in any format, including computer-assisted storage and readout, in which the issue and/or volume may be reproduced by the publisher or by its licensed agencies.

Preparation of Copy

1. Type *all* copy—including indented matter, footnotes, tables, and references—*double-spaced*, allowing generous margins at top, bottom, and sides of page. Please do not break words at ends of lines or justify right-hand margins. Indicate italics by underlining only. Number all footnotes, including acknowledgment footnote (which should be "1").
2. Type each table on a separate page. Refer to each table in numerical order in the text. Prepare tables without vertical lines. Tables should not contain more than 20 two-digit columns or the equivalent.
3. Clarify all mathematical symbols (e.g., Greek letters) with words in the margins of the manuscript.
4. Draw figures on white paper with india ink. Original or glossy print of professionally drawn figure will be required if manuscript is accepted.
5. Include a brief abstract (not more than 100 words) summarizing the findings.
6. *Four copies* of the manuscript and abstract, on white paper, must be submitted. Manuscripts should be addressed to the Editor of the AMERICAN JOURNAL OF SOCIOLOGY, 1130 East 59th Street, The University of Chicago, Chicago, Illinois 60637. *Manuscripts are not returned to authors.* Enclose a self-addressed stamped postcard, containing the manuscript title, to obtain acknowledgment of manuscript receipt. No other acknowledgment will be sent.

7. Each article submitted to the *AJS* must be accompanied by a check or money order for \$15.00, payable to The University of Chicago Press in U.S. currency or its equivalent by money order or bank draft. Papers will not be reviewed until this fee has been paid. The submission fee will be waived for papers solely by student authors. Submissions from students must be accompanied by a faculty member's certification of their student status. Citizens of countries with restrictions on the export of U.S. dollars may request waiver of the submission fee.

Format of References in Text: All references to monographs, articles, and statistical sources are to be identified at the appropriate point in the text by last name of author, year of publication, and pagination where appropriate, all within parentheses. Footnotes are to be used only for substantive observations. Specify subsequent citations of the same source in the same way as the first one; do not use "ibid.," "op. cit.," or "loc. cit."

1. When author's name is in the text: Duncan (1959). When author's name is not in text: (Gouldner 1963).
2. Pagination follows year of publication: (Lipset 1964, pp. 61-65).
3. For more than three authors, use "et al." For institutional authorship, supply minimum identification from the beginning of the complete citation: (U.S. Bureau of Census 1963, p. 117).
4. With more than one reference to an author in the same year, distinguish them by use of letters (*a*, *b*) attached to the year of publication: (1965*a*).
5. Enclose a series of references with a single pair of parentheses, separated by semicolons.

Format of References: List all items, including machine-readable data files (MRDF), alphabetically by author (providing the full list of multiple authors) and, within author(s), by year of publication, in an appendix titled "References." Examples follow:

Davis, James Allan. 1978. *General Social Survey, 1972-1978: Cumulative Data* (MRDF). NORC ed. Chicago: NORC. Distributed by Roper Public Opinion Research Center, New Haven, Conn.

Davis, K. 1963*a*. "Social Demography." Pp. 124-37 in *The Behavioral Sciences Today*, edited by Bernard Berelson. New York: Basic.

—. 1963*b*. "The Theory of Change and Response in Modern Demographic History." *Population Index* 29:345-66.

Goode, W. J. 1967. "The Protection of the Inept." *American Sociological Review* 32:5-19.

Moore, Wilbert E., and Arnold S. Feldman. 1960. *Labor Commitment and Social Change in Developing Areas*. New York: Social Science Research Council.

Sanford, Nevitt, ed. 1962. *The American College*. New York: Wiley.

U.S. Bureau of the Census. 1970. *Census of Population and Housing 1970, Summary Statistic File 4H: U.S.* (MRDF). DUALabs ed. Washington, D.C.: Bureau of the Census. Distributed by DUALabs, Rosslyn, Va.

Weber, Max. (1921) 1968. "Society's Problems." Pp. 12-16 in *Economy and Society*, edited by Guenther Roth and Claus Wittich. New York: Bedminster.

Media Discourse and Public Opinion on Nuclear Power: A Constructionist Approach¹

William A. Gamson
Boston College

Andre Modigliani
University of Michigan

Media discourse and public opinion are treated as two parallel systems of constructing meaning. This paper explores their relationship by analyzing the discourse on nuclear power in four general audience media: television news coverage, newsmagazine accounts, editorial cartoons, and syndicated opinion columns. The analysis traces the careers of different interpretive packages on nuclear power from 1945 to the present. This media discourse, it is argued, is an essential context for understanding the formation of public opinion on nuclear power. More specifically, it helps to account for such survey results as the decline in support for nuclear power before Three Mile Island, a rebound after a burst of media publicity has died out, the gap between general support for nuclear power and support for a plant in one's own community, and the changed relationship of age to support for nuclear power from 1950 to the present.

Atoms for peace. Your friend, the atom. Electricity too cheap to meter. Dr. Spock is worried. The Clamshell Alliance. *The China Syndrome*. Images of cooling towers at Three Mile Island. Chernobyl is everywhere. These are nuggets from a public discourse on nuclear power that most of us instantly recognize.

Nuclear power, like every policy issue, has a culture. There is an ongoing discourse that evolves and changes over time, providing inter-

¹ The research reported here has been supported by the National Science Foundation grants SES-801642 and SES-8309343. We had helpful comments on earlier drafts from William Hoynes, Elihu Katz, Sharon Kurtz, Charlotte Ryan, Howard Schuman, David Stuart, and the anonymous reviewers. Requests for reprints should be sent to William A. Gamson, Department of Sociology, Boston College, Chestnut Hill, Massachusetts 02167.

© 1989 by The University of Chicago. All rights reserved.
0002-9602/90/9501-0001\$01.50

American Journal of Sociology

pretations and meanings for relevant events. An archivist might catalog the metaphors, catchphrases, visual images, moral appeals, and other symbolic devices that characterize this discourse. The catalog would be organized, of course, since the elements are clustered; we encounter them not as individual items but as interpretive *packages*.

On most policy issues, there are competing packages available in this culture. Indeed, one can view policy issues as, in part, a symbolic contest over which interpretation will prevail. This cultural system has a logic and dynamic of its own. Packages ebb and flow in prominence and are constantly revised and updated to accommodate new events. The process by which this issue culture is produced and changed needs to be accounted for in its own right, regardless of any claims that one might make about its causal effect on public opinion.

Parallel to this cultural level is a cognitive one of individuals making sense of the same issue. Individuals bring their own life histories, social interactions, and psychological predispositions to the process of constructing meaning; they approach an issue with some anticipatory schema, albeit sometimes with a very tentative one. Most public opinion studies focus on the aggregate outcomes of this process—that is, attitudes for and against particular policies—and on how such attitudes change over time. The findings suggest which schemata are shared and the relative popularity of different competitors.

Both levels of analysis involve the social construction of meaning. By examining discourse and public opinion as parallel systems, we deliberately avoid making certain causal assumptions. We do not, in this paper, argue that changes in media discourse *cause* changes in public opinion. Each system interacts with the other: media discourse is part of the process by which individuals construct meaning, and public opinion is part of the process by which journalists and other cultural entrepreneurs develop and crystallize meaning in public discourse.

A full exploration of this interaction between media discourse and opinion formation requires an analysis of both systems over several issues. In this paper, our attempt is more modest: to show how changing media discourse on nuclear power provides an essential context for interpreting a variety of survey results on nuclear power. But our argument on how media discourse and public opinion interact will not be clear without a fuller explication of our underlying model.

The Nature of Media Discourse

Public discourse is carried on in many different forums. Rather than a single public discourse, it is more useful to think of a set of discourses that interact in complex ways. On an issue such as nuclear power, there is the

Nuclear Power

specialist's discourse using journals and other print media aimed at those whose professional lives involve them in the issue. There is the largely oral discourse used by officials who are directly involved in decision-making roles on the issue and by those who attempt to influence them. There is the challenger discourse, providing packages that are intended to mobilize their audiences for some form of collective action.

General audience media, then, are only some of the forums for public discourse on an issue. If one is interested in predicting policy outcomes, they are not necessarily the most important forums. But if one is interested in public opinion, then media discourse dominates the larger issue culture, both reflecting it *and* contributing to its creation. Journalists may draw their ideas and language from any or all of the other forums, frequently paraphrasing or quoting their sources. At the same time, they contribute their own frames and invent their own clever catchphrases, drawing on a popular culture that they share with their audience.

The media, in this model, serve a complex role. They are, on the one hand, part of the process by which issue cultures are produced. Because their role is believed to be so central in framing issues for the attentive public, they are also, to quote Gurevitch and Levy (1985, p. 19), "a site on which various social groups, institutions, and ideologies struggle over the definition and construction of social reality." General audience media are not the only forums for public discourse, but, since they constantly make available suggested meanings and are the most accessible in a media-saturated society such as the United States, their content can be used as the most important indicator of the general issue culture.

Media packages.—We suggested earlier that media discourse can be conceived of as a set of interpretive packages that give meaning to an issue. A package has an internal structure. At its core is a central organizing idea, or *frame*, for making sense of relevant events, suggesting what is at issue. "Media frames," Gitlin (1980, p. 7) writes, "largely unspoken and unacknowledged, organize the world both for journalists who report it and, in some important degree, for us who rely on their reports." This frame typically implies a range of positions, rather than any single one, allowing for a degree of controversy among those who share a common frame. Finally, a package offers a number of different condensing symbols that suggest the core frame and positions in shorthand, making it possible to display the package as a whole with a deft metaphor, catchphrase, or other symbolic device.²

² We distinguish framing devices that suggest how to think about the issue and reasoning devices that justify what should be done about it. The five framing devices are (1) metaphors, (2) exemplars (i.e., historical examples from which lessons are drawn), (3) catchphrases, (4) depictions, and (5) visual images (e.g., icons). The three reasoning devices are (1) roots (i.e., a causal analysis), (2) consequences (i.e., a particular type of

American Journal of Sociology

To illustrate, consider a package about the use of nuclear power to generate electricity, one we label *progress*.³

If the electric chair had been invented before the electric light, would we still be using kerosene lamps? There has always been resistance to technological progress by nervous Nellies who see only the problems and ignore the benefits. Resistance to nuclear energy development is the latest version of this irrational fear of progress and change, the expression of modern pastoralists and nuclear Luddites. Certainly nuclear energy development is not free of problems, but problems can be solved, as the history of technological progress shows. The failure to develop nuclear power will retard our economic growth and make us renege on our obligation to the poor and to future generations. If coercive utopians prevent us from moving ahead now with nuclear energy, the next generation is likely to be sitting around in the dark blaming the utilities for not doing something this generation's officials would not let them do.

This package frames the nuclear power issue in terms of the society's commitment to technological development and economic growth. Frames should not be confused with positions for or against some policy measure. While this package is clearly pronuclear, there is ample room for disagreement within the overall frame—for example, on what type of reactors should be built. Not every disagreement is a frame disagreement; differences between (say) Republicans and Democrats or "liberals" and "conservatives" on many issues may reflect a shared frame. Nor can every package be identified with a clear-cut policy position. On almost any issue, there are packages that are better described as ambivalent than as pro or con.

Packages, if they are to remain viable, have the task of constructing meaning over time, incorporating new events into their interpretive frames. In effect, they contain a story line or, to use Bennett's (1975) term, a scenario. The *progress* package, for example, must be able to deal with the accidents at Three Mile Island (TMI) and Chernobyl, providing them with a meaning that is plausible and consistent with the frame. If

effect), and (3) appeals to principle (i.e., a set of moral claims). A package can be summarized in a signature matrix that states the frame, the range of positions, and the eight different types of signature elements that suggest this core in a condensed manner. For a fuller presentation of this part of the model, see Gamson and Lasch (1983).

³ We follow the convention of presenting packages as indented quotations, although they are in fact a combination of paraphrasing and direct quotes from multiple sources. The acid test of a statement of a package should be its acceptance by an advocate that the statement is a fair one. We attempt to meet this test by relying on the language of advocates and sponsors, deriving it from their pamphlets and other writings. In this instance, we paraphrase or quote materials from the Atomic Industrial Forum, the Edison Electric Institute, the Committee on Energy Awareness, and the pronuclear writings of neoconservatives (see Nisbet 1979; McCracken 1977, 1979).

the event is not one that the scenario predicts or expects, this only challenges the ingenuity and suppleness of the skillful cultural entrepreneur.

How does one account for the development of this package and its competitors over time and especially for their relative prominence in media discourse? Our model treats the content of the discourse as the outcome of a value-added process. As an illustration of this concept, consider the example of automobile production. Each stage—the mining of iron ore, smelting, tempering, shaping, assembling, painting, delivering, selling—adds its value to the final product. Furthermore, these stages may be thought of as determinants that, in combination, *specify* the final outcome. In this sense, they “explain” or account for whatever it is that is finally produced.⁴

The production of issue cultures can be thought of as such a process. The model postulates three broad classes of determinants that combine to produce particular package careers: cultural resonances, sponsor activities, and media practices.

A. Cultural resonances: Not all symbols are equally potent. Certain packages have a natural advantage because their ideas and language resonate with larger cultural themes. Resonances increase the appeal of a package; they make it appear natural and familiar. Those who respond to the larger cultural theme will find it easier to respond to a package with the same sonorities. Sncw and Benford (1988) make a similar point in discussing the “narrative fidelity” of a frame. Some frames “resonate with cultural narrations, that is, with the stories, myths, and folk tales that are part and parcel of one’s cultural heritage.”⁵

The *progress* package benefits by its resonances with a larger cultural theme of technological progress. Few would question the appeal of a “technofix” for a wide variety of problems in American society. As Williams writes in commenting on American values, “‘Efficient’ is a word of high praise in a society that has long emphasized adaptability, technological innovation, economic expansion, up-to-dateness, practicality, expediency, ‘getting things done’ ” (1960, p. 428). The inventor is a central

⁴ The economic model of *value added* has been used most prominently in sociology by Smelser (1963) in his *Theory of Collective Behavior*. In spite of the influence of this work, the general linear model so dominates the thinking of most American sociologists that they find it difficult to think in value-added terms, immediately attempting to translate such models into the language of dependent and independent variables. But it is confusing rather than helpful to think of an automobile as the dependent variable, while mining, smelting, painting, and delivery are considered independent variables.

⁵ They also use the term “frame resonance,” but to refer to the link between culture and cognition—i.e., to connect the content of a frame and the response of an audience member. In contrast, we use the terms “cultural resonance” and “narrative fidelity” to link different parts of the cultural system—i.e., to connect symbols on a specific issue with more enduring cultural themes.

American Journal of Sociology

cultural hero—embodied in the myths about Benjamin Franklin and Thomas Edison. Mastery over nature is the way to progress: good old American ingenuity and know-how.

It is useful to think of themes dialectically. There is no theme without a countertheme. The theme is conventional and normative; the counter-theme is adversarial and contentious. But both are rooted in American culture, and both can be important in assessing the outcome of any specific symbolic contest.

American culture also contains a countertheme that is skeptical of, or even hostile to, technology. To quote Emerson, “Things are in the saddle and ride mankind.” Harmony with nature rather than mastery over it is stressed. We live on a “small planet.” Our technology must be appropriate and in proper scale. There is an ecosystem to maintain, and the more we try to control nature through our technology, the more we disrupt its natural order and threaten the quality of our lives. Thoreau at Walden Pond is also part of the American cultural heritage.

Much of popular culture features the countertheme: Chaplin’s *Modern Times*, Huxley’s *Brave New World*, and Kubrick’s *2001* and countless other films about technology gone mad and out of control, a Frankenstein’s monster about to turn on its creator. If *progress* benefits by its resonance with the theme, two of its competitors, *runaway* and *soft paths* (discussed below), draw much of their symbolism from different parts of the countertheme.

Since cultural themes remain constant, it may be unclear how they can help us to explain changes in the ebb and flow of packages in media discourse. Resonances are the earliest stage in the value-added process. A package’s resonances, we argue, facilitate the work of sponsors by tuning the ears of journalists to its symbolism. They add prominence to packages by amplifying the effect of sponsor activities and media practices.

B. Sponsor activities: Much of the changing culture of an issue is the product of enterprise. Packages frequently have sponsors, interested in promoting their careers. Sponsorship is more than merely advocacy, involving such tangible activities as speech making, interviews with journalists, advertising, article and pamphlet writing, and the filing of legal briefs to promote a preferred package.

These sponsors are usually organizations, employing professional specialists whose daily jobs bring them into contact with journalists. Their jobs breed sophistication about the news needs of the media and the norms and habits of working journalists. Indeed, many of these professionals began as journalists before moving to public relations jobs. As Sigal (1973, p. 75) points out, professional sponsors adjust “their thinking to newsmen’s conventions. They talk the same language.”

The sponsor of a package is typically an agent who is promoting some

collective rather than personal agenda. These agents frequently draw on the resources of an organization to prepare materials in a form that lends itself to ready use. Condensing symbols is the journalist's stock-in-trade. Smart sources are well aware of the journalist's fancy for the apt catchphrase and provide suitable ones to suggest the frame they want.

For nuclear power, as on most issues, public officials are often important sponsors. The Atomic Energy Commission and its successor agencies, the Nuclear Regulatory Commission and the Department of Energy, have been important sponsors of the *progress* package. Their efforts have been supplemented by industry groups such as the Atomic Industrial Forum, the Edison Electric Institute, and the Committee on Energy Awareness. A neoconservative advocacy network has helped to articulate and spread this package through its journals.

Social movement organizations are also important sponsors in this framing process. Snow and Benford (1988, p. 198) point out their role as "signifying agents" that are actively engaged in the production of meaning: "They frame . . . relevant events and conditions in ways that are intended to mobilize potential adherents and constituents, to garner bystander support, and to demobilize antagonists."

Major sponsors of antinuclear packages include environmental groups such as Friends of the Earth, consumer protection groups such as Critical Mass, professional groups such as the Union of Concerned Scientists, and direct-action groups such as the Clamshell Alliance. Gamson (1988) argues that the antinuclear movement—through a combination of direct action and more conventional political action—so changed media discourse that the accidents at TMI and Chernobyl were given significantly different frames than they would have received in an earlier discourse context.

C. Media practices: That sponsors are active does not imply that journalists are passive. Journalists' working norms and practices add considerable value to the process. A number of students of American news organizations have argued that journalists unconsciously give official packages the benefit of the doubt. In some cases, official assumptions are taken for granted, but even when they are challenged by sponsors of alternative packages, it is these competitors that bear the burden of proof. A weaker form of this argument is that journalists make official packages the starting point for discussing an issue.

Various observers have noted how subtly and unconsciously this process operates. Halberstam (1979, p. 414) describes how Walter Cronkite's concern with avoiding controversy led to his acceptance of the assumptions underlying official packages: "To him, editorializing was going against the government. He had little awareness, nor did his employers want him to, of the editorializing which he did automatically by uncon-

American Journal of Sociology

sciously going along with the government's position." In addition to this tendency to fall into official definitions of an issue, journalists are especially likely to have routine relationships with official sponsors. Most American reporting is the product of ongoing news routines.⁶

Other media norms and practices in the United States—particularly the balance norm—favor certain rivals to the official package. In news accounts, interpretation is generally provided through quotations, and balance is provided by quoting spokespersons with competing views. In the commentary provided by syndicated columnists and cartoonists, norms of balance generally prevail at the aggregate level. While an individual columnist is not expected to provide more than one package, a range of "liberal" and "conservative" commentators are used to observe this norm.

The balance norm is, of course, a vague one, and the practices that it gives rise to favor certain packages over others. Organized opposition to official views is a necessary condition for activating the norm, which, once invoked, encourages the tendency to reduce controversy to two competing positions—an official one and (if there is one) the alternative sponsored by the most vested member of the polity. In many cases, the critics may share the same unstated, common frame as officials.

The balance norm, however, is rarely interpreted to include challenger packages, even when no other alternative is available. Tuchman (1974, p. 112) argues that balance in television news "means in practice that Republicans may rebut Democrats and vice versa," but that "supposedly illegitimate challengers" are rarely offered the opportunity to criticize government statements. Instead, she suggests, reporters search for an "establishment critic" or for a "responsible spokesman" whom they have themselves created or promoted to a position of prominence."

But challengers can have an important indirect effect on media discourse. Their own preferred packages may be ignored, but they create the conditions for more established critics to gain media prominence. On nuclear power, as Gamson (1988, p. 235) puts it, "When demonstrators are arrested at Seabrook, phones ring at the Union of Concerned Scientists."

⁶ Sigal (1973) examined over 1,000 stories from the *New York Times* and the *Washington Post* and classified the channels by which the information reached the reporter. *Routine* channels included official proceedings, press releases, press conferences, and scheduled official events. *Informal* channels included background briefings, leaks, nongovernmental proceedings, and reports from other news organizations. Finally, *enterprise* channels included interviews conducted at the reporter's initiative, spontaneous events that a reporter observed firsthand, independent research, and the reporter's own conclusions or analysis. He found that only about one-quarter of the stories came from enterprise channels, while routine channels accounted for almost 60%.

In sum, packages succeed in media discourse through a combination of cultural resonances, sponsor activities, and a successful fit with media norms and practices. Public opinion influences this process indirectly through journalists' beliefs, sometimes inaccurate, about what the audience is thinking. Many journalists straddle the boundary between producers and consumers of meaning. These journalists—editorial writers, cartoonists, opinion columnists, and the like—are not engaged in constructing accounts of raw happenings. They observe and react to the same media accounts, already partly framed and presented in a context of meaning, that are available to other readers and viewers. In their commentary on an issue, they frequently attempt to articulate and crystallize a set of responses that they hope or assume will be shared by their invisible audience.

The Nature of Public Opinion

"Is there anyone out there not thinking about this nightmare of the nuclear age, talking about it, learning from it?" began NBC's Tom Brokaw in one of his daily updates on the Chernobyl nuclear accident. How do ordinary citizens come to understand a complex issue such as nuclear power? On many issues, people encounter relevant phenomena directly rather than through mass-media accounts. They try to understand events in light of what touches their lives. But few of us have experiences with nuclear power.

Ball-Rokeach and DeFleur's "dependency theory" (1976, 1982) suggests that the role of the media in the process of constructing meaning will vary from issue to issue. On some issues, the audience has little experience by which to judge media-generated images and meanings; concerning other issues, they have a great deal. The media-dependency hypothesis suggests that the relative importance of media discourse depends on how readily available meaning-generating experiences are in people's everyday lives.

Even in the apparently limiting case of nuclear power, however, there are more relevant experiences than one might think. Take the issue of evacuation plans in the event of nuclear accidents. In Boston, for example, virtually everyone is aware of how a single automobile accident on the central artery can paralyze traffic in and out of the city for hours. They can bring this type of practical knowledge to bear in evaluating the realism of nuclear evacuation plans.⁷ Even concerning nuclear power, then, media dependency is far from complete.

⁷ This example is drawn from research in progress in which we construct peer groups to discuss nuclear power and other issues.

American Journal of Sociology

However dependent the audience may be on media discourse, they actively use it to construct meaning and are not simply a passive object on which the media work their magic. Swidler (1986, p. 273) invites us to think of culture "as a 'tool kit' of symbols, stories, rituals, and world-views, which people may use in varying configurations to solve different kinds of problems." The problem in this case is to make sense of public affairs. Media discourse, we argue, provides many of the essential tools.

Note that this model of the relationship of media discourse and public opinion does not argue that media discourse causes public opinion to change. But if packages and their elements are essential tools, then it makes a considerable difference that some are more readily available than others. Making sense of the world requires an effort, and those tools that are developed, spotlighted, and made readily accessible have a higher probability of being used.

THE NUCLEAR POWER DISCOURSE

Our analysis focuses on national media discourse and, more specifically, on television network news, major newsmagazine accounts, editorial cartoons, and syndicated columns. We take all relevant material on nuclear power during the limited time periods that we sample. This includes the network evening news broadcasts on ABC, CBS, and NBC; *Time*, *Newsweek*, and *U.S. News and World Report*; and a "saturation" sample of editorial cartoons and syndicated opinion columns that includes virtually all those published during the sample periods.

The cartoons and opinion columns are drawn from a sample of the 10 largest-circulation daily newspapers in each of five regions. We calculate how close we are to saturation by examining each wave of five newspapers and calculating the percentage of new entries that they yield. We define our set as complete when the index of new entries is below 20% for two successive waves. For example, the last 10 newspapers we sampled in 1953 yielded 29 columns but only two that were not already included.

We use our media sample as an indicator of the issue culture that people draw on to construct meaning. We do not, of course, assume that people have watched all three television networks or read the 50 newspapers from which we draw our cartoons and opinion columns. But we do assume that the national issue culture that our media sample reflects is accessible to those who try to make sense of nuclear power, either directly through national media or through local media and personal conversations about the issue.

Ideally, we would want a continuous record of media discourse, with no time gaps. But we also want a record that transcends the idiosyn-

crasies of a given medium. Together, these requirements present overwhelming practical obstacles, and some compromises are necessary.

Our time sampling focuses on what Chilton (1987) calls "critical discourse moments," which make the culture of an issue visible. They stimulate commentary in the media by sponsors and journalists. With continuing issues such as nuclear power, journalists look for "pegs"—that is, topical events that provide an opportunity for broader, more long-term coverage and commentary. These pegs provide us with a way of identifying those time periods in which issue packages are especially likely to be displayed.

The events we sample typically create some perturbation. Sponsors feel called upon to reassert their preferred packages and to interpret the latest development in light of them. This increases the efficiency of our search by focusing our efforts on periods when commentary is especially dense. But by sampling in this fashion, we end up with a small series of snapshots of media discourse at irregular intervals instead of a movie, which we would prefer. The analysis below necessarily reflects this limitation.

Part of our presentation is based on a systematic content analysis that uses standard coding and reliability techniques. For this, we used a three-digit code that breaks packages down into specific idea elements. For example, within the *progress* package, the code provides such categories as "Underdeveloped nations can especially benefit from peaceful uses of nuclear energy," "Nuclear power is necessary for maintaining economic growth and our way of life," and "Nuclear power opponents are afraid of change." The coder looks for a specific idea such as one of the above rather than making a global judgment on which package it represents. Of course, since the first digit groups subcodes by the overall package, coders find the package distinctions useful in knowing where to search. Two independent coders were used on a sample of material, and distinctions among code categories were not maintained whenever the reliability failed to reach 80%.⁸

Some of our analysis, especially of visual imagery, is more qualitative and interpretive. Here we attempt to present enough rich textual material so that readers can form their own independent judgments on the validity of our argument. Whenever possible, we draw on other analysts who have examined some aspect of nuclear discourse. Since the story is frequently in the details, this necessarily implies a large number of concrete illustrations and a somewhat lengthy presentation.

⁸ For a copy of the complete code used, write to the first author. Further details on the sampling, compiling, and coding process and the reliabilities involved are presented in Gamson and Modigliani (1987, pp. 171–74).

American Journal of Sociology

1. The Age of Dualism: From Hiroshima through the 1960s

The culture of nuclear power has been indelibly marked by Hiroshima and Nagasaki. Public awareness begins with the images of sudden, enormous destruction, symbolized in the rising mushroom cloud of a nuclear bomb blast. Even when discourse focuses on the use of nuclear reactors to produce electricity, the afterimage of the bomb is never far from the surface.

Boyer's rich analysis of American nuclear discourse from 1945 to 1950 shows how rapidly these images of unlimited destruction became central. H. V. Kaltenborn, in his NBC evening news broadcast reporting on the first atomic bomb, told his radio audience that "For all we know, we have created a Frankenstein! We must assume that with the passage of only a little time, an improved form of the new weapon we use today can be turned against us" (Boyer 1985, p. 5). *Life* magazine, with over 5 million circulation, devoted much of its August 20, 1945, issue to the bomb, with full-page photographs of the towering mushroom clouds over Hiroshima and Nagasaki. The language that accompanied these frightening images was equally ferocious. Today, fears of extinction, as Boyer points out, "seem so familiar as to be almost trite, but it is important to recognize how quickly Americans began to articulate them" (1985, p. 15).

The *progress* package on nuclear energy, described above, was just as quick off the mark. A dualism about nuclear energy is part of its core. Boyer points to the either/or structure of so many post-Hiroshima pronouncements: "Either civilization would vanish in a cataclysmic holocaust, or the atomic future would be unimaginably bright" (1985, p. 125). "We face the prospect either of destruction on a scale which dwarfs anything thus far reported," said the *New York Times* in an editorial a day after Hiroshima, "or of a golden era of social change which would satisfy the most romantic utopian." A Philip Wylie article in the September 1945 *Collier's* was titled "Deliverance or Doom." By September 1945, Dwight Macdonald was already calling such a view an "official platitude": "The official platitude about Atomic Fission is that it can be a Force for Good (production) or a Force for Evil (war), and that the problem is simply how to use its Good rather than its Bad potentialities" (Macdonald 1945, p. 58).

Boyer argues that the faith expressed in the atom's peacetime promise was "part of the process by which the nation muted its awareness of Hiroshima and Nagasaki and of even more frightening future prospects" (1985, p. 127). Not only was it an "anodyne to terror," but it also helped to assuage any lingering discomfort over the destruction that America had already wrought with the fearsome atom. A peace-loving America should embrace the challenge of making the atom "a benevolent servant"

Nuclear Power

to produce for humankind "more comforts, more leisure, better health, more of real freedom [and] a much happier life" (Waymack 1947, p. 214). Atomic Energy Commission (AEC) chairman Lewis Strauss contributed a phrase that became a permanent part of the issue culture when he told the National Association of Science Writers in 1954 that "It is not too much to expect that our children will enjoy in their homes electrical energy too cheap to meter."

Not all the discourse that Boyer reviews was equally optimistic. There were certainly cautious skeptics challenging the utopian claims. But this is a debate within a frame, a disagreement over how fast and how easily the promise of nuclear energy will be realized. As long as the issue is framed as a choice between atoms for war and atoms for peace, it is hard to see who could be against nuclear power development.

Nuclear dualism remained essentially unchallenged for the next quarter century. On December 8, 1953, President Eisenhower addressed the United Nations on nuclear power, presenting what media discourse labeled his "atoms for peace" speech. In it, he proposed to make American nuclear technology available to an international agency that would attempt to develop peaceful uses of nuclear energy.

We sampled media material for the two weeks after the UN speech and for a similar period in February 1956, following the issuance of a citizens committee report on the future of nuclear energy.⁹ The Eisenhower speech came at the height of the Cold War and in the midst of the McCarthy era. Much of the discourse that followed focused less on nuclear power and more on how Eisenhower's clever one-upmanship had embarrassed an obstructionist and militaristic Soviet Union. Nevertheless, we were able to identify 21 columns, 16 cartoons, and 4 news-magazine accounts that did address the issue of nuclear power per se.¹⁰

The *progress* package remains unchallenged throughout this sample of materials. The either/or structure of nuclear dualism is strongly represented. The dominant metaphor is a road that branches into two alternative paths—one leading to the development of weapons of destruction, the other to the eradication of human misery. Again, there are optimists and cautious skeptics who warn that the technological problems in tap-

⁹ The report was issued by a special panel of nine prominent citizens appointed by the Congressional Joint Committee on Atomic Energy. It depicted a future in "which the nation would add immeasurably to its material resources, extend its atomic bounty to the backward and improve the physical well-being of peoples everywhere." Unfortunately, the report stimulated very little media commentary; our 1950s sample is overwhelmingly composed of items from the 1953 discourse.

¹⁰ No television was available for these early periods. The Vanderbilt Television Archive began recording the evening news broadcasts of the three major networks on August 5, 1968.

American Journal of Sociology

ping this energy source for human betterment are formidable and far from solved. But no opposition to nuclear power development is presented, and no alternative package is ever offered.

In the late 1950s and early 1960s, a movement against the atmospheric testing of nuclear weapons called public attention to the long-range dangers of radiation. Milk, "nature's most nearly perfect food," as the dairy industry put it, was found to contain strontium 90. A famous SANE ad warned the public that "Dr. Spock is worried."

Some of this increased awareness about radiation dangers spilled over into concern about nuclear reactors. Local controversies developed over the licensing of some of them, including the Enrico Fermi reactor near Detroit. But these controversies remained local and largely disappeared after the Limited Test Ban Treaty of 1963 ended atmospheric testing of weapons and radiation concerns receded. By the mid-1960s, the nuclear energy industry was enjoying a wave of new orders and no public opposition.

One measure of the dominance of the *progress* package at this time was the lack of attention paid to a serious nuclear accident at the Fermi reactor outside Detroit in the fall of 1966. On October 5, the cooling system failed and the fuel core underwent a partial meltdown.¹¹ The automatic shutdown, or "scram," system failed to operate, and, alerted by alarms signaling the leak of radiation into the containment building, operators shut the plant down manually. As far as we know, there was no radiation leak into the atmosphere, but the shutdown did not remove the major danger of a disastrous secondary accident during the following six months, when people tried to figure out what had happened and to remove the damaged fuel. Fuller (1975) likens the process to "look[ing] inside a gasoline tank with a lighted match." During the danger period, plans for the evacuation of a million or more people were discussed by officials but deemed impractical and unnecessary. By almost any reckoning, the Fermi accident was extremely serious.

Local journalists and officials were notified that something was wrong, but there the story sat, unreported. More than five weeks after the accident, the *New York Times* carried a story on what it labeled a "mishap" at the Fermi reactor.¹² There was nothing in the least alarming in the *Times* account. Walker Cisler, the president of Detroit Edison and the leading force behind the construction of the Fermi reactor, was quoted as saying, "If all goes well, we could start again shortly after the first of the year."¹³

¹¹ We rely here on the detailed account of the Fermi accident by Fuller (1975).

¹² The *New York Times*, November 13, 1966.

¹³ The breeder reactor at Fermi was eventually abandoned, although a conventional light water reactor (Fermi 2) was later built next to it.

A General Electric official classified what happened as "a minor perturbation," and a reassuring report from the Atomic Industrial Forum was duly noted.

No critic of nuclear power was quoted in the belated *Times* report on the Fermi accident. Indeed, it would have taken great enterprise to have found such a critic in 1966. In effect, there was no significant anti-nuclear-power discourse during this era.¹⁴ Nuclear power was, in general, a nonissue. *Progress* remained the dominant package, so taken for granted in the little public discourse that existed that it required no explicit defense.

2. The Rise of an Antinuclear Discourse: The 1970s to TMI

By the time of the Three Mile Island accident in 1979, media discourse on nuclear power reflected an issue culture in flux. *Progress* was still the most prominent package, but its earlier hegemony had been destroyed.

The much-touted "energy crisis" of the 1970s stimulated the articulation of a second major pronuclear package, *energy independence*. This package drew a pronuclear meaning from the Arab oil embargo of 1973:

The lesson is how dependence on foreign sources for vitally needed energy can make the United States vulnerable to political blackmail. Nuclear energy must be understood in the context of this larger problem of energy independence. To achieve independence, we must develop and use every practical alternative energy source to imported oil, including nuclear energy. Nuclear energy, plus domestic oil, natural gas, and coal, remain the only practical alternatives to a dangerous and humiliating dependence on foreign and, particularly, Middle Eastern sources. These foreign sources are unstable and unreliable and are likely to make unacceptable political demands. Do we want to be dependent on the whims of Arab sheiks? Ultimately, independence is the cornerstone of our freedom.

This addition to the pronuclear arsenal was more than offset by other developments that stimulated the rise of an antinuclear discourse. First, nuclear dualism had been seriously eroded even among many keepers of the faith. With the advent of the Carter administration, proliferation of nuclear weapons became a presidential priority issue. To deal with the proliferation problem, Carter tried to promote stronger international control over the spread of nuclear technology, including reactor technology. Although a strong supporter of nuclear power generally, he turned against the breeder reactor lest the plutonium it produced be diverted to weapons use. Atoms for peace and atoms for war no longer appeared to

¹⁴ From August 5, 1968, through the end of 1969, there was only one 15-second item on nuclear power on the television evening news programs of the three major networks (see Media Institute 1979).

American Journal of Sociology

be such separate paths. Subliminal mushroom clouds had begun to gather over even official discourse on the issue.

More important, the dualism was being undermined because of the safety issue. If a serious accident that releases large amounts of radiation into the atmosphere is possible at a nuclear reactor, then the destructive potential of this awesome energy is not confined to bombs.

A broad coalition of anti-nuclear-power groups raised the safety issue but as part of a number of different packages. The environmental wing, epitomized by Friends of the Earth, offered a *soft paths* package:

Split wood, not atoms. Nuclear energy presents us with a fundamental choice about what kind of society we wish to be. Do we wish to continue a way of life that is wasteful of energy, relies on highly centralized technologies, and is insensitive to ecological consequences? Or do we want to become a society more in harmony with its natural environment?

Nuclear energy relies on the wrong kind of technology—centralized and dangerous in the long run to the earth's ecology. We need to pursue alternative, soft paths. We should change our way of life to conserve energy as much as possible and to develop sources of energy that are ecologically safe and renewable, and that lend themselves to decentralized production—for example, sun, wind, and water. Small is beautiful.¹⁵

Other groups, epitomized by the Ralph Nader organization Critical Mass, offered a more populist, anticorporate package, *public accountability*:

If Exxon owned the sun, would we have solar energy? The root of the problem is the organization of nuclear production by profit-making corporations, which minimizes accountability and control by the public. Spokesmen for the nuclear industry are motivated to protect their own economic interests, not the public interest. One cannot rely on what they say. Company officials are frequently dishonest, greedy, and arrogant. Who killed Karen Silkwood?

The nuclear industry has used its political and economic power to undermine the serious exploration of energy alternatives. Public officials, who are supposed to monitor the activities of the industry, are all too often captives of it. They function more to protect the industry than to protect the public.

Finally, the antinuclear movement, through organizations such as the Union of Concerned Scientists, offered a more pragmatic, cost-benefit package, *not cost effective*. A liturgy of unsolved problems and delays are cited, leading to the conclusion that:

When one compares the costs and benefits of nuclear energy with the alternatives, it makes a poor showing. Nuclear power, through nobody's fault in particular, has turned out to be a lemon, and it is foolish to keep pouring

¹⁵ See Lovins (1977) for a particularly influential articulation of this package.

Nuclear Power

good money after bad by supporting the continued development of nuclear energy.

Media coverage of nuclear power accelerated rapidly in the mid-1970s. The Media Institute study (1979) of network television news reveals a burst of coverage at the time of Earth Day in 1970, followed by very little through 1974. Coverage then tripled in 1975 and doubled again the following year. Except for a temporary decrease in 1978, it continued to increase up to the time of TMI. In the first three months of 1979, before TMI, the networks ran 26 stories related to nuclear power.

We sampled two two-week periods in the 1970s. The first, in 1973, followed a major energy speech by Nixon. Occurring in the midst of Watergate, it drew little commentary and yielded only three opinion columns. The second period, in 1977, coincided with two events: Carter's efforts at gaining international support for controlling the spread of nuclear technology and the arrest and detention for two weeks of more than 1,400 antinuclear demonstrators who occupied the site where the Seabrook, New Hampshire, nuclear reactor was being constructed. This sample produced fifteen television segments, two newsmagazine accounts, six cartoons, and an additional five opinion columns.

Our media samples represent two different forms of discourse. Television and newsmagazines present accounts rather than explicit commentary. The accounts, of course, tell a story and frame the information presented, particularly in the headings, leads, and closings. Numerous interpretive comments are sprinkled through the accounts in the form of quotations from sources or, in the case of television, excerpts from interviews. Cartoons and opinion columns are billed as commentary and are freer of such constraints. They are especially useful since their packages are more explicit and easier to extract.

Television.—All the television coverage centers on the collective action by the Clamshell Alliance at Seabrook and its aftermath. New Hampshire Governor Meldrim Thomson blessed the "Clam" with a major social control error. The 1,414 demonstrators who were arrested were not, as expected, released on their own recognizance. Instead, they were charged with criminal trespass and asked to post bail ranging from \$100 to \$500, which they refused. They were then held in five national guard armories for 12 days, creating a continuing national television story. Each of the networks ran segments on five different days, although sometimes merely a short update.

The television story is about a dyadic conflict between Governor Thomson and his allies and the Clamshell Alliance over whether or not the Seabrook reactor will be completed. The central question in this story

American Journal of Sociology

is of who will win, and, hence, there is very little direct commentary about nuclear power. But the coverage does address one central issue that divides packages: What kind of people are against nuclear power?

For a deaf television viewer, the answer would seem to be people who wear backpacks and play frisbee. All three networks feature these images in more than one segment. One sees beards and long hair, bandanas, "no nuke" buttons, and people playing guitars and doing needlepoint. Outside the courthouse, after the demonstrators have been released, we see happy family reunions, with many children.

These visual images do not have a fixed meaning. People approach them with some anticipatory schema. A *progress* sympathizer may see frivolous flower children and environmental extremists who look as if they will not be happy until they turn the White House into a bird sanctuary. A *soft paths* viewer may see loving, caring, earthy people who are socially integrated and concerned about our shared environment. But these two are the *only* packages that really suggest an interpretation of these images, and those who use neither are likely to be especially susceptible to the meanings suggested by the accompanying words.

Here there are network differences. The CBS and NBC coverage leaves the work to the viewer, but ABC offers its own interpretation and it suggests a *progress* package. We are told that these are the same kind of people who were involved in antiwar demonstrations, "demonstrators in search of a cause." The network allows two members of the Clam to speak for themselves, quoting their determination to win while ignoring their reasons ("We have to stop it at any cost"). Such quotations fit nicely with the dyadic conflict frame, but no package on nuclear power is displayed. No interviews or quotes from the protesters were used by NBC.

Only CBS made any attempt to present the demonstrators' frame. In introducing its May 2 segment, it reported that the Clamshell Alliance opposed the plant because "they say it is dangerous and a threat to the coastal marine life." In a later segment, we hear Harvey Wasserman, a spokesman for the group, claim the antinuclear movement as an antiwar movement: "We are fighting the war that is being waged against the environment and our health." Later, in the same segment, the threat to marine life is mentioned along with safety concerns as reasons for the protest. Beyond these three utterances, three general expressions of determination by demonstrators are quoted, but no other antinuclear packages are suggested.¹⁶

¹⁶ We should note, out of fairness to the networks, that the Media Institute examined "outside sources" quoted in 10 years of coverage before TMI. They found that antinuclear sources received considerably more airtime than pronuclear ones. The Union of Concerned Scientists finished first by a wide margin (with 6:18). Ralph Nader (3:34)

Newsmagazines.—*Time* and *Newsweek* ran stories on Seabrook, again with a dyadic conflict frame and little commentary about nuclear power per se. The demonstrators are presented relatively sympathetically. Both magazines mention their commitment to nonviolence, and *Newsweek* adds their exclusion of drugs, weapons, and fighting. The photographs reinforce the television images of backpackers; *Newsweek* calls them scruffy and mentions frisbee playing, guitar playing, and reading Thoreau. Environmental concerns and the threat to marine life are mentioned but not elaborated. *Time* also quotes the publisher of the *Manchester Union Leader*, William Loeb, who likened the Clam to "Nazi storm troopers under Hitler," but characterizes him in a discrediting way as an "abrasive conservative."

A number of antinuclear movement spokespersons are quoted, including Harvey Wasserman, Ralph Nader, and representatives from Friends of the Earth, the Sierra Club, and the National Resources Defense Council. But none of the selected quotes suggest a frame on nuclear power; instead, they focus exclusively on the strategy of direct action and whether the demonstrators will succeed. The *Newsweek* story, in particular, leaves the impression of internal division among movement organizations.

There is a largely implicit *progress* frame, reflected in the full separation of the controversy from concerns about nuclear weapons (a dualism that was largely accepted by the Clam as well) and by statements in both magazines suggesting the necessity and inevitability of nuclear power as an energy source. No antinuclear package is displayed beyond the faint hint of *soft paths* implied by the mention of safety and environmental concerns.

Cartoons.—None of the seven cartoons comment on Seabrook as such. Three focus on Carter's efforts to control the spread of nuclear technology, two on the failure of some nuclear plants to account for all of their plutonium and enriched uranium,¹⁷ one on the administration's energy plans in general, and one on the safety issue. There is no *progress* in this set, but they do not all suggest an antinuclear package. Four of the seven

edged out the Atomic Industrial Forum (3:26) for second place. This finding should be tempered by two observations: (1) official sources, such as the Nuclear Regulatory Commission, who are not included in this analysis, often present a pronuclear package, and (2) critics may be quoted without their packages on nuclear power being displayed, as the Seabrook coverage demonstrates.

¹⁷ Publicity over the mysterious 1974 death of Karen Silkwood had brought to light that the Kerr-McGee fuel processing plant in Oklahoma where she worked was unable to account for 40 pounds of plutonium.

American Journal of Sociology

suggest a new package, *runaway*, whose position on nuclear power is fatalistic or resigned more than opposed:

We did not understand what we were getting into with nuclear power. We thought we could harness it to maintain our standard of living. Now we are committed to it and will sooner or later have to pay a price of unknown dimension. We have unleashed it but we no longer can control it. Nuclear power is a powerful genie that we have summoned and are now unable to force back into its bottle; a Frankenstein's monster that might turn on its creator. Nuclear power is a time bomb, waiting to explode. Nuclear energy is not simply one among several alternative energy sources but something more elemental. It defies a cost-benefit analysis. Radiation is invisible and one may be exposed without knowing it; its harmful effects may not show up right away but may strike suddenly and lethally at some later point. Radiation can create grotesque mutants. In a religious version, humans have dared to play God in tampering with the fundamental forces of nature and the universe. He who sows the wind, reaps the whirlwind.

Runaway has an antinuclear flavor, to be sure, but the gallows humor by which it is frequently expressed suggests resignation and fatalism more than opposition. "Grin and bear it" is more the message than "No nukes." Once the genie is out of the bottle, it is too late. Not surprisingly, *runaway*, unlike all the earlier packages, has no organized sponsor attempting to further its career. But it is strongly implied in four of the seven cartoons.

Two of the seven cartoons express an anticorporate theme suggesting the *public accountability* package. The striking thing about this cartoon set, compared with television and newsmagazine accounts, is the implicit rejection of nuclear dualism in six of the seven. They are about nuclear power and not weapons, but nuclear power plants are themselves a time bomb.

Opinion columns.—The eight columns offer a sharp contrast to the cartoons. Here there is a strong dualism, sometimes quite explicit, although the separation between atoms for peace and atoms for war is no longer as simple as in the 1950s. There is a dilemma, James Reston suggests, over "how to develop nuclear power for peaceful purposes and at the same time restrain its development as an instrument of war." The columns address general energy problems and Carter's efforts to control nuclear proliferation in a context that fully accepts the necessity and inevitability of nuclear power development. The issue is not whether to go ahead with nuclear power but how fast and in what ways. Four of the eight columns also emphasize *energy independence* as a strong secondary theme.

Only one column focuses on the Seabrook action, linking the Clam with the 1960s images of antiwar protestors—scruffy beards, longish hair, and braless women. Some of them "really don't know what they are protesting" writes Jeremiah Murphy, "and—far worse—don't care."

There is only one faint suggestion in the entire set of columns of any antinuclear package or even of the fatalistic *runaway*.¹⁸

This review of media discourse before TMI provides a mixed picture. With the exception of cartoons, there is virtually no display of any anti-nuclear package, but the confident dualism of an earlier era has become uneasy at best. *Progress* is represented in the acceptance of nuclear power development as necessary and inevitable. But the discourse clearly recognizes it as controversial, even if one can gain only a vague awareness of how nuclear opponents think about the issue.

The editorial cartoonists present a very different picture—one in which the themes of *runaway* and *public accountability* are dominant. Perhaps their greater distance from active sponsors and the debunking inherent in their medium makes them more resistant to the official package. In any event, they presage the packages that will come into general prominence only after TMI.

The apogee of antinuclear discourse in the effect on popular consciousness came with the release, a few scant weeks before TMI, of a major Hollywood film, *The China Syndrome*. The film numbered among its stars Jane Fonda, an actress so closely identified with the antinuclear movement that pronuclear groups used her as a symbol of it. The themes emphasized by the film suggest the *public accountability* and *runaway* packages, but its most important achievement was to provide a concrete, vivid image of how a disastrous nuclear accident could happen. Of course, it was just a movie.

3. Life Imitates Art: From TMI to Chernobyl

As events unfold, each package must offer an interpretation that is consistent with its story line. Although it is always possible to do this, the result is sometimes labored, particularly if the event is, from the standpoint of the package, unexpected. Consider how the *progress* package handles TMI and Chernobyl:

TMI showed that the safety systems worked even in the face of a string of improbable errors. A total core meltdown was prevented, and most of the radiation released never breached the containment building. Furthermore, we learned from the experience and have improved safety even more. Chernobyl has equally sanguine lessons. It shows the wisdom of the American nuclear industry in building large fortified containment structures as a safety precaution. Nuclear reactors in the United States have multiple

¹⁸ Richard Strout, in discussing other countries' distrust of Carter's motives in trying to curtail breeder reactors, has them wondering whether the United States is "trying to create a capitalistic monopoly of nuclear fuel for itself." Our coders included this under the *public accountability* category on corporate greed.

American Journal of Sociology

protective barriers, called "defense in depth." American nuclear reactors cannot be compared with their Soviet counterparts any more than their political systems are comparable. Furthermore, even in this most serious of accidents, it turns out that initial claims of thousands killed reflected mere hysteria, egged on by antinuclear activists.

Events, as the Fermi accident illustrates, do not speak for themselves. By 1979, a *progress* interpretation was forced to compete with others that were saying that a serious nuclear accident could and probably would happen. No complicated interpretation is necessary for a prophecy fulfilled.

We sampled the media for two weeks after both TMI and Chernobyl.¹⁹ Our TMI sample yielded 53 television segments, 6 newsmagazine accounts, 71 cartoons, and 56 opinion columns. The accounts, as we noted above, are less explicit in their framing of nuclear power as such. Their stories on TMI center on two central questions: (1) What is it like to be living next to TMI? Since there are many other reactors, there is a more general question implied here: What is it like to be living near a nuclear reactor that has had an accident? (2) Is the situation at TMI under control? Again, there is a more general story, especially as the immediate TMI crisis subsides: Is this technology under control?

Television.—The situation at TMI was a continuing story that dominated the coverage of all three networks during the sample period. Visually, we were treated to repeated aerial shots of the reactor site, making the special shape of a nuclear cooling tower a familiar visual symbol for the first time. The use of this icon by cartoonists began with TMI, where it frequently took on an ominous tone.

Nimmo and Combs (1985) suggest that ABC in particular used the cooling towers as visual reinforcement for a *runaway* package that permeated its coverage and provided its central story line: "In the gothic romance, the threat to peace, tranquility, and happiness is embodied in a forbidding structure overlooking the community of simple folk. . . . Dr. Frankenstein's castle in Transylvania, in a bucolic countryside above a quaint village, is the classic setting." They argue that ABC's footage and camera angles played on such imagery, "especially on days when ABC correspondents did stand-up reports with the plant's massive cooling towers, enveloped in mist, looming in the background. . . . Aerial shots, too, captured a technological intruder in a rural setting" (pp. 69–70).

There is rare use of the mushroom cloud symbol. The Media Institute study (1979) found only four instances of its use in the more than 10-year period it covered—including the TMI period in our sample. We found it

¹⁹ The Chernobyl samples include only television and newsmagazine accounts. The assembly of cartoons and opinion columns involves a much more complicated data-gathering process—beyond our resources at that stage of the research.

Nuclear Power

only twice in our sample, but, in addition, two of the three networks ran an Atomic Energy Commission film of bomb tests in the late 1950s in Nevada.²⁰ The film shows houses being obliterated in an awesome blast, followed by the familiar rising mushroom cloud. Direct images of nuclear destruction are rare, but high frequency may be unnecessary for effect if the association is already present in the schemata of many viewers and the images are sufficiently vivid and evocative.

Seven years later, with the accident at Chernobyl, nuclear power again dominated the television evening news. All three networks ran nightly stories for the entire period of our two-week sample. Visually, there were many repeats of imagery from TMI coverage but with several new additions. The most striking new image involved frequent footage of radiation detectors being used to check people and food. There were 15 different instances of such visual reminders of the invisible danger theme, well distributed among the three networks. In addition, all three showed American or European antinuclear protestors, with a total of eight such instances. The protestors' signs reminded viewers that "Chernobyl can happen here" or "Chernobyl is everywhere." Radiation, in television graphics, is almost invariably red—often in the form of a pulsating red dot to show an untamed reactor or a spreading red streak to represent the flow of fallout. Of the 12 occasions on which such graphics were employed, only once was another color used (in this case, white).

For both events, there is much visual filler of little interest for the framing of nuclear power. There are many talking heads, didactic summaries of points being made by announcers and interviewees, and graphics to illustrate a technical discourse on nuclear reactors.

On the audio, two themes in particular dominate the accounts: official confusion and the scary, invisible effects of nuclear radiation. As they are developed, these themes give a powerful boost to the *runaway* and *public accountability* packages. There were, in this coverage, 99 utterances that expressed idea elements central to a package on nuclear power. Figure 1 shows the distribution among the set of six.

The once dominant *progress* has shrunk to a mere 18% and frequently has a grudging and defensive tone. For example, NBC quotes Secretary of Energy James Schlesinger conceding that TMI was an "unfortunate occurrence and the reaction to it will not be beneficial, save that it may permit us to better understand some of the plant operations and that the NRC will be able to institute measures that will reduce risks."²¹

²⁰ This film was pegged to coverage of congressional hearings on charges that the U.S. government had lied to local residents in Nevada about the dangers of radioactive fallout from its atomic tests.

²¹ NBC News, March 30, 1979.

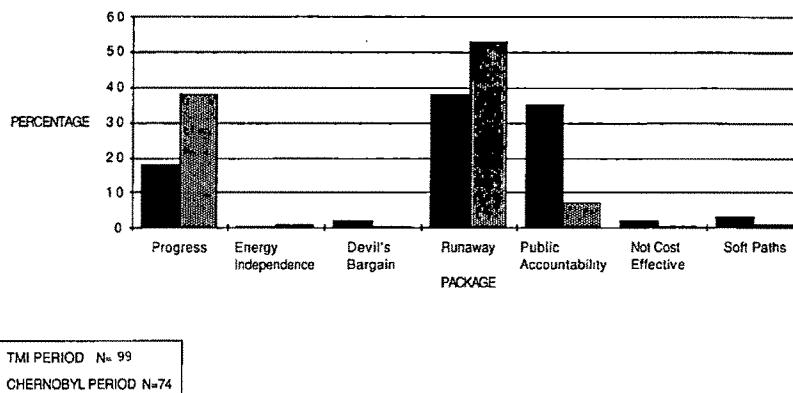


FIG. 1.—Nuclear power packages in television utterances

Nimmo and Combs (1985) suggest that *progress* may still be prominent at a more aggregate level, but only in CBS's coverage. Its formula, repeated in each nightly update, is a warning of danger and an explanation that shows the nature of the problem and of officials managing it. "Experts," "scientists," "officials," or "technicians" are shown to be dealing with the danger and its consequences as best they can in a difficult situation. The general story line of CBS, Nimmo and Combs suggest, is "an adventure tale in the tradition of 'disaster averted' movies. . . . In such dramas, responsible people take concerted action to bring an unfortunate situation under control" (p. 68).

The two most prominent packages are clearly *runaway* and *public accountability*, with 38% and 35%, respectively. The former is displayed primarily through two central ideas, which together account for almost three-fourths of its total: (a) the overconfidence theme: officials in charge of nuclear energy may think they have it under control but they really do not, and (b) the hidden danger theme: radiation effects are invisible and delayed, so that one may not know the true harm done until many years later.

The interpretation of official confusion is less benign in the *public accountability* package. The emphasis here is less on self-deception and more on deliberate misleading of the public by the nuclear industry. We distinguish here a weak and a strong form of the package. The strong version, which includes about 30% of its total, suggests that profits are emphasized at the expense of public safety, that government regulation is ineffective because public officials function as promoters of the industry, or that industry interests work against providing full protection and information to the public. The weak version suggests merely some culpability by company managers, with negative consequences for the public or

consumers. No analysis of reasons for such culpability is suggested beyond general incompetence, stupidity, laxness, or overconcern with public image.

The almost total absence of *energy independence, not cost effective*, and *soft paths* is quite striking. All these packages, although differing in their conclusions, frame nuclear power in terms of a broader energy issue, comparing it with alternative sources. Implicitly, television coverage emphasizes the uniqueness of this source and those features that make comparison difficult or impossible.

The coverage of TMI saw the emergence of a new frame for understanding nuclear power, characterizing it as a Faustian *devil's bargain*:

So nuclear power turns out to be a bargain with the devil. There are clear benefits such as inexhaustible electricity and an energy supply that does not depend on the whims of OPEC. But sooner or later, there will be a terrible price to pay. We are damned if we do and damned if we don't. And the deeper we get in, the harder it is to get out.

Devil's bargain is a package that conflates pronuclear packages with *runaway*. It is a thoroughly ambivalent package—both for and against nuclear power. Figure 1, which uses utterances as a unit, does not do justice to the prominence of this package because it is more frequently expressed by combinations of utterances in the same rhetorical sequence. When pronuclear claims and *runaway* elements occur sequentially, for example, one might argue that it has been implicitly invoked. This sequence in fact occurred in five of the 18 television segments with three or more codable utterances, and the frame was made explicit in a sixth segment.

The Chernobyl coverage produced an additional 74 utterances implying a core frame on nuclear power. As figure 1 indicates, there is an apparent comeback for *progress*, which rebounds to 38%. Almost two-thirds of these *progress* utterances, though, are represented by the claim that American reactors have safety features—such as reinforced containment structures—that were lacking in the Chernobyl reactor, making a similar accident unlikely or impossible. Another 14% of them consist of claims, mostly emanating from Soviet sources, that the American media were exaggerating the seriousness of the Chernobyl accident. Only once is *progress* expressed in a clearly positive way through an assertion of the benefits of nuclear power.

Runaway remains the leading package, with many images of what Dan Rather called the “nuclear nightmare of a reactor gone wild.” It is represented in claims that, in spite of some differences in reactor technology, a Chernobyl could occur here, and in images of a spreading “silent killer”—an invisible cloud of radioactive fallout. *Public accountability* is dis-

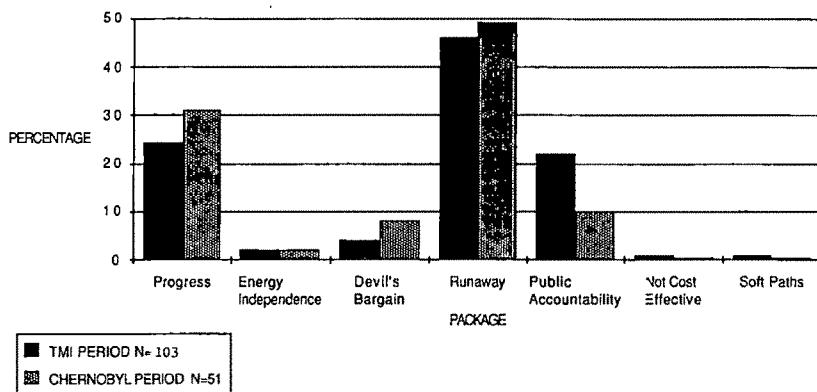


FIG. 2.—Nuclear power packages in newsmagazine utterances

played mainly by invoking comparison with earlier official dissembling in the United States at the time of Three Mile Island. The *devil's bargain* frame is never made explicit, but the sequence of a claimed benefit for nuclear power juxtaposed with *runaway* imagery occurs in two of the nine segments with three or more codable utterances.

Newsmagazines.—The pattern here is quite similar. Figure 2 shows the distribution of 103 utterances displaying central ideas in one or more of our six packages. *Progress* does somewhat better in quantity, but, as with television, it is a beleaguered faith that is expressed. *Time*, for example, quotes Alvin Weinberg, introducing him as a nuclear advocate and pro-nuclear author who believes that the alternatives to this source are "so crummy that we probably should in a cautious way continue this nuclear enterprise."²² This is still *progress*, but it has evolved quite a bit from "too cheap to meter."

Runaway is by far the most prominent package, but some caution is necessary in interpreting this. More than two-thirds of the utterances that evoke it focus on the overconfidence theme that the developers of nuclear power have overestimated their control and do not know as much about what they are doing as they have led us to believe.

Public accountability drops off in quantity from its television prominence, but when it is displayed, the strong form of it is presented more fully, accounting for almost half the coded utterances (compared with one-third of television displays of this package). *Newsweek*, for example, quotes Jane Fonda: "We can never be safe in the hands of utility executives whose financial interests require them to hide the truth from the public."

²² *Time*, April 9, 1979, p. 20.

Nuclear Power

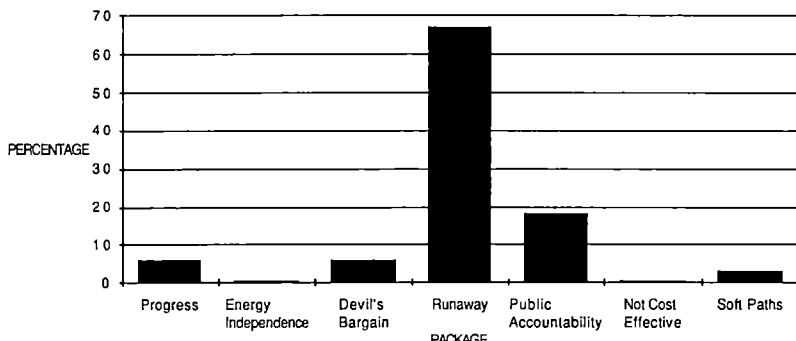


FIG. 3.—Nuclear power packages in cartoons, TMI period ($N = 67$)

The prominence of *devil's bargain* is, again, underrated by using utterances as a unit. Both *Time* and *Newsweek* make it explicit twice, and all three display it implicitly by juxtaposing claimed benefits of nuclear power with *runaway* themes. The remaining packages have very low prominence in newsmagazine discourse.

Figure 2 also shows the package scores for coverage of Chernobyl. *Runaway* leads with almost half of the codable utterances, and, again, *progress* is displayed defensively, mainly by comparisons of the safety of Soviet and American nuclear power plants that malign the former. *Devil's bargain* is again expressed both implicitly and explicitly in two of the three magazine accounts.

Cartoons.—While *runaway* fares well in accounts, it receives its fullest expression in cartoons, where it dominates the discourse. As figure 3 indicates, more than two-thirds of the cartoons express it in one way or another. There are two ideas in particular that account for almost half of the *runaway* cartoons.

The first is well illustrated by the Don Wright cartoon (fig. 4). The joke is on those who think they have this technology under control. The audience can see that they do not, but the "nucleocrats" are themselves unaware and foolishly overconfident. The second idea is expressed through gallows humor about nasty nuclear surprises, as in the Larry Wright cartoon (fig. 5). We suggest that such humor expresses the fatalism that is at the core of this package—the acceptance of nuclear power as an inevitable, uncontrollable fact of life combined with anxiety about the unknowable disasters that may spring from it. Gallows humor, as Hodge and Mansfield suggest (1985, p. 210), is a way of "distracting the unthinkable so that it can be turned on its head, and subjected to a sense of control."²³

²³ A caveat about these results is in order. We made two important coding decisions

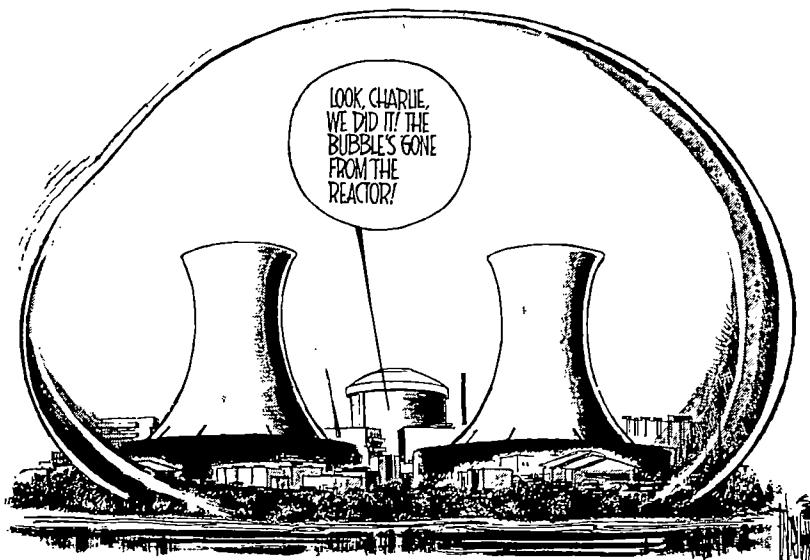


FIG. 4.—Cartoon by Don Wright (*Boston Herald American*, April 6, 1979)



FIG. 5.—Cartoon by Larry Wright (*Detroit News*, April 8, 1979)

Progress appears only four times (6%) and always in the same form—through mocking the overreaction of antinuclear hysterics. Figure 6 expresses the idea most clearly, suggesting that people who oppose nuclear energy are like those who would have opposed the invention of the ox cart in prehistoric times.

Opinion columns.—Figure 7 shows the percentage of columns in which

that may have led to underestimating the *public accountability* package. There were six cartoons playing on the coincidence of *The China Syndrome* and TMI. Where there was no explicit emphasis on corporate culpability, we have treated these as *runaway*. There were also five cartoons depicting overconfident nuclear officials but with the deliberateness of their deception left ambiguous. Again, we ended up including these as part of *runaway* rather than *public accountability*.

Nuclear Power



FIG. 6.—Cartoon by Jeff MacNelly (*Chicago Tribune*, April 10, 1979). (Reprinted by permission of the Tribune Company Syndicates, Inc.)

each package was displayed.²⁴ A fuller range of packages is displayed in this discourse than in the other media samples—every package registers at least 10%. Furthermore, packages are more richly elaborated in this sample, not merely suggested by a passing comment or brief quote.

Progress does slightly better here than in the other samples but also encounters a good deal of mockery as its rivals are presented. Art Buchwald, for example, bemoans the selfishness of many Americans living near nuclear plants, who are unwilling to make sacrifices so that other people, hundreds of miles away, can be assured that “their toasters and electric coffee makers will work. . . . Unfortunately, they can’t appreciate that with any form of electricity there is a tradeoff, and it’s impossible to have cheap nuclear power without a few noxious gases, an occasional hydrogen bubble, a meltdown and possibly an explosion which could make one or two states uninhabitable for 50 or a hundred years.” Buchwald assures us that his own views on nuclear power depend on which way the wind is blowing. When it is away from Washington, D.C., he is pronuke, but on mornings when the wind is blowing from TMI, he tells his wife, “I think Jane Fonda is right.”²⁵

²⁴ Columnists frequently display more than one package to balance or evaluate arguments pro and con. Hence, the percentages in fig. 4 add up to more than 100%.

²⁵ Buchwald’s column, “As the Wind Blows,” appeared in newspapers on April 10, 1979.

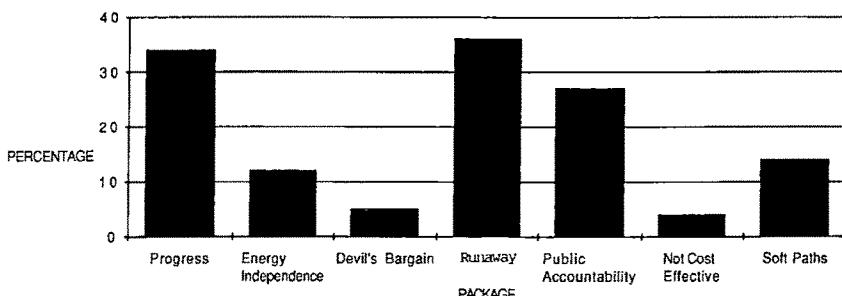


FIG. 7.—Nuclear power packages in opinion columns, TMI period ($N = 56$). Percentages total more than 100% because some columns display more than one package.

The *soft paths* package is more frequently and fully displayed in this sample than anywhere else, appearing in a positive way in one-seventh of the columns. It generally takes the form of advocating a major effort at conservation and the development of soft-path energy alternatives, sometimes combined with belittling comments on American addiction to energy-consuming gadgetry.

Overall.—The picture that emerges is a newly dominant *runaway* package. The pronuclear *progress* is still a prominent contender, but it is beleaguered and defensive, a far cry from the 1950s version. Most important for understanding public opinion, the dominant package in media discourse is fatalistic. When its impressive totals are combined with the thoroughly ambivalent *devil's bargain*, it is clear that any overall characterization of media discourse as pro- or anti-nuclear-power necessarily obscures this central fact.

SURVEY DATA ON NUCLEAR POWER

Questions have been asked about nuclear power on sample surveys for many years, and there are a number of useful reviews of this material.²⁶ We select here certain results that, we argue, can be fully understood only in the context of media discourse on the issue.

1. Questions have been asked concerning nuclear power in general as well as about a plant's being built nearby. As one would expect, there are consistently higher levels of opposition to building a nuclear power plant near one's own community than to nuclear power development in general. This discrepancy suggests the *nimby* position on nuclear power: "OK, but not in my backyard." Substantial change in the percentage

²⁶ See esp. Mazur (1988); Nealey, Melber, and Rankin (1983); Mitchell (1980); and Freudenberg and Baxter (1983).

Nuclear Power

saying they support or oppose local plants results from slight changes in question wording, making it difficult to estimate the exact size of this group, but it appears to be at least 15% and is probably much higher.

2. On the question of opposition to a local plant, a substantial shift occurred even *before* the accident at TMI. In 1971, only 25% were opposed to the building of a nuclear plant in their communities.²⁷ By 1978, before TMI, opposition to local nuclear facilities had jumped to 45% and actually exceeded support for the first time. By 1980, opposition had grown to 63% while support for a local plant had dropped to only 25% (compared with 57% in 1971). Finally, a 1986 Gallup poll conducted for *Newsweek* in the aftermath of Chernobyl shows opposition to a local plant reaching 70%.²⁸ The trend toward increasing opposition before the TMI accident is less clear when people were asked about supporting nuclear power development in general.

3. At first blush, one might think that TMI was a watershed event that destroyed public confidence in nuclear power. From February 1979, just before Three Mile Island, to April 1979, just after TMI, opposition to nuclear power rose by 14% while support fell by 11%. Mazur's (1981) analysis, however, shows a rapid recovery to previous levels of support. Yes, there was a sharp temporary increase in opposition to nuclear power with the flood of publicity about the TMI accident, but when the media spotlight was turned off, public opinion rebounded almost immediately to pre-TMI levels. Even more impressive, the same rebounding effect was replicated at the release of the Kemeny Commission report on the accident six months later. Again, there was a sharp increase in media coverage, accompanied by a sharp drop in support for nuclear power. And, again, there was the same rebound to previous levels once the media spotlight was turned off.

The rebound, however, is never quite complete. When public opinion is viewed over a 15-year period beginning in the early 1970s, TMI looks like little more than a small blip, which slightly accelerated a secular trend against nuclear power.

4. There is a striking reversal in the relationship of age to support for nuclear power. Back in 1950, Fisher et al. (1951, p. 76) found that "youn-

²⁷ Twenty years earlier, Fisher, Metzner, and Darsky (1951) asked people about "the establishment of an atomic plant near their residence" and got the identical result of 25% opposed. The authors of this early study show unusual prescience, noting that favorable attitudes toward nuclear power rest on a thin faith: "If there were any sort of evidence that not even the experts quite understand or could control this tremendous source of energy, . . . attitudes might sharply incline to the negative. . . . In all likelihood, it would take but one highly dramatic and well publicized event . . . to upset the faith" (p. 102).

²⁸ *Newsweek*, May 12, 1986, p. 30.

ger respondents are definitely more positive in their acceptance of atomic energy and this positive reaction decreases with age." By the 1970s, this result had been largely reversed. In surveys taken between 1975 and 1980, general opposition to nuclear power averaged 40% in the 18–25 age group, dropping to 37% for the 26–35 group and to about 30% for those over 36.²⁹

5. One final result worth noting is that those who have reviewed multiple surveys have noted the sensitivity of responses to small differences in question wording and the context of the question in the interview (Freudenberg and Baxter 1983; Nealey et al. 1983; Mitchell 1980). It is not clear whether this volatility is really higher than on other issues, but it struck these observers as significant when they attempted to distill the results of many different surveys.

Interpretation

How does our analysis of media discourse provide a necessary context for understanding these survey results? Imagine a member of the public, old enough to remember Hiroshima and the age of nuclear dualism, trying to make sense of the issue of nuclear power. Let us assume, further, that the issue has only moderate to low salience for our hypothetical citizen except on those occasions when it is given high priority in the media.

Back in the 1950s, she would almost certainly have used a *progress* schema³⁰ to understand nuclear power; no other frame was available. Until the mid-1970s, she would have had little reason to think about the issue at all. We can reasonably assume that, when she encountered media discourse, her anticipatory schema remained *progress*.

At this point, her personal exposure to the issue culture through either the media or other discourses, her enduring predispositions, and her interpersonal interactions would all have played a role in the modification of her working schema on nuclear power. Many paths were possible, but the nature of the media discourse suggests that certain ones were especially likely.

In the mid-1970s, the discourse overwhelmingly accepted the inevitability and societal commitment to nuclear power development, but there was, at the same time, a significant erosion of nuclear dualism combined

²⁹ Surveys going back to the 1950s show a large and consistent gender gap, with women much more opposed to nuclear power. The explanation for this almost certainly lies outside media discourse. Nelkin (1981) has a particularly insightful treatment of the issues.

³⁰ Schema and package are parallel concepts. We use "schema" when referring to the level of individual cognition and package when referring to the cultural or discourse level.

Nuclear Power

with public controversy about the safety of nuclear reactors. In cartoon discourse, in particular, *progress* was supplanted by *runaway* and *public accountability*, a harbinger of their general prominence after TMI.

We would hypothesize that many older people during this period began to conflate their pronuclear *progress* schema with *runaway* themes to produce some version of the ambivalent *devil's bargain*.³¹ Clear antinuclear packages are rarely encountered in the national media we sampled during this period. Without some independent exposure to other forums of discourse, it is difficult to see how someone would arrive at one of our antinuclear schemata.

The media discourse stimulated by TMI, we hypothesize, accelerated a shift from *progress* to *runaway* and *devil's bargain* as the most popular schemata among the attentive public. Displays of the old faith, when they occurred, emphasized the necessity and inevitability of nuclear power—idea elements that can be incorporated into these alternatives. At the same time, the theme of a technology out of control, defying its alleged masters, was repeated again and again. Images of false confidence and apparent deception by the nuclear managers abounded. Gallows humor about mutants, hidden radiation, and nuclear catastrophes dominated the cartoon sample, and the same themes came up more soberly in opinion columns as well.

Members of the public who paid attention only after TMI should have encountered this stream of media discourse in a different way. Not having participated in the issue culture when *progress* dominated, they should have been more likely to adopt an unambivalent antinuclear schema. Hence, we would expect less outright opposition and more ambivalence among people who became politically conscious before the 1970s.

After TMI, we suspect that the majority of those who appear to support nuclear power were, in fact, ambivalent. The behavioral psychologist's approach-avoidance conflict is the prototype. When food is associated with a severe electric shock, the rat is both attracted and repelled by the same object. In the case of nuclear power, it is not merely a matter of recognizing arguments on both sides but of experiencing simultaneous tendencies to approach and avoid it.

Some proportion of those who were already ambivalent at the time of TMI would have shifted to an antinuclear schema, particularly *not cost effective* or *public accountability*. *Soft paths* was rarely displayed in

³¹ We are talking here about those members of the public who had some minimal awareness of the issue. For many people (perhaps a majority) before TMI, nuclear power had insufficient salience for them to make any effort to make sense of it. And even since Chernobyl, when the size of this inattentive group has undoubtedly become smaller, one should not assume that everyone has a working schema on nuclear power.

media discourse except in the attenuated form of general environmental and conservation concerns and interest in the solar energy alternative. A person would have had to move beyond national media discourse to encounter this package in a fully developed form.

Media discourse after Chernobyl reinforced both tendencies: from pro-nuclear *progress* to the fatalism of *runaway* or the ambivalence of *devil's bargain*, and from ambivalence to outright opposition. Today, we suspect, a pure *progress* schema is rare among the public; the significant division that remains is between those who continue to accept the necessity or inevitability of nuclear power, inherent in the *devil's bargain* and *runaway* packages, and those who unambivalently oppose it, weaving fragments of the available discourse into some overall antinuclear schema.

These speculations about public thinking on nuclear power are very difficult to test directly with existing survey data. Consider the dilemma that a typical survey question on nuclear power presents to respondents with a *devil's bargain* schema: "In general, do you favor or oppose the building of more nuclear plants in the United States?" How does one respond if one believes that nuclear power is a necessary fact of life but that sooner or later there will be an enormous price to pay? Does one answer favor, oppose, or not sure? Any of these alternatives is consistent with a *devil's bargain* schema.

Nevertheless, the survey results cited above provide indirect evidence for the argument. For someone with a *devil's bargain* schema, general support combined with local opposition provides a neat solution to the dilemma of what to do about nuclear power: the *nimby* position. If the nuclear plant is not too close, the avoidance valence is less pronounced and nuclear power becomes a more attractive object.

We do not argue that people will come to the *nimby* position through seeing it advocated in media discourse and adopting it. On the contrary, one would be hard put to find it, since it is not openly espoused by any spokesperson for the government, nuclear industry, or antinuclear movement. But media discourse increasingly supports the conflated *devil's bargain* package, both directly and by offering *runaway* on top of decades of uncontested *progress*.

Widespread ambivalence can also account for the temporary decline in apparent support for nuclear power during moments of peak media coverage, followed by a partial rebound to former levels when the media attention subsides. When the media discourse gives the dangers a temporary imminence, the force of the avoidance vector increases and ambivalence is temporarily resolved as opposition. When media attention turns elsewhere, the balance of forces is restored for those who retain an ambivalent schema.

The evidence for change in how age influences opposition to nuclear power adds further support to our argument. In an earlier era, young people were least opposed to nuclear power because they were especially susceptible to the appeal of the *progress* package. Today, having become middle-aged, they retain the long-term influence of this discourse and are more likely to be ambivalent than opposed. Young people today, never having experienced the era in which *progress* reigned unchallenged, are more likely to be opposed rather than ambivalent.

Our model also suggests an interpretation of why there should be so much volatility of response with slight changes in question wordings. The questions are difficult, if not impossible, for an ambivalent respondent to answer. Of course, such volatility may simply mean the absence of *any* stable schema rather than a stable and conflicting one. But it is, at a minimum, consistent with ambivalence.

Finally, there is some more direct evidence on widespread ambivalence in two surveys reviewed by Nealey et al. (1983). In one, conducted in October 1979, respondents were asked independently about both "benefits" and "harmful consequences" from building more nuclear power plants. If we count the 37% who could think of no benefits as antinuclear and the 22% who could think of no harmful consequences as pronuclear, this gives us an estimate of 41%.³²

Another survey in July 1979 yielded a similar estimate. All respondents, regardless of attitude, were asked what they saw as the major advantages and disadvantages of nuclear power. This form yielded a slightly higher antinuclear percentage, with 42% unable to name any advantage and only 14% unable to name any disadvantage. This leaves a balance of 44% who were ambivalent.

CONCLUSION

We have argued here that public opinion about nuclear power can be understood only by rooting it in an issue culture that is reflected and shaped by general audience media. The conventional method of assessing public opinion through responses to survey questions with fixed categories has two major drawbacks for our constructionist model, making it difficult to test our argument directly. First, it obscures ambivalence and disguises the presence of schemata that produce no clear-cut position for or against. Second, it blurs the distinction between people with nonat-

³² This question was asked only of the 91% who had "ever heard or read about controversies over nuclear power plants." Ambivalence, as we indicated above, is more than simply knowing arguments on both sides. Hence, such figures should be regarded as crude estimates of upper limits on the number of those who are ambivalent.

American Journal of Sociology

titudes—that is, with no working schema on an issue—and those with schemata that do not fit comfortably in a pro or anti category.

One of the major lessons of the 1964 Converse article "The Nature of Belief Systems in Mass Publics" was to warn against the tendency to impose elite dichotomies such as "liberal" and "conservative" on a mass public whose beliefs are not organized by such dimensions. Similarly, the classification of the public into "hawks" and "doves" concerning Vietnam war attitudes made it difficult to identify those whose schema led them to a "win or get out" position. This lesson is ignored so easily, we submit, because of a methodological tradition that assumes the task is to array relevant publics on a pro-con dimension.

By framing issues for people through the question asked and the pre-coded response categories offered, the method assumes a shared frame on nuclear power. But a constructionist model begins by calling this assumption into question and examining it. A proper constructionist methodology for assessing public opinion must do more to make the underlying schemata visible in some fashion, preferably by allowing us a glimpse of the thinking process involved.³³ Only by methods that elicit more of the interpretive process will we be able to see the extent to which different media packages have become part of the public's tool kit in making sense of the world of public affairs.

REFERENCES

Ball-Rokeach, Sandra J., and Melvin DeFleur. 1976. "A Dependency Model of Mass Media Effects." *Communication Research* 3:3-21.

———. 1982. *Theories of Mass Communication*, 4th ed. New York: Longman.

Bennett, W. Lance. 1975. *The Political Mind and the Political Environment*. Lexington, Mass.: Heath.

Boyer, Paul. 1985. *By the Bomb's Early Light*. New York: Pantheon.

Chilton, Paul. 1987. "Metaphor, Euphemism, and the Militarization of Language." *Current Research on Peace and Violence* 10:7-19.

Converse, Philip E. 1964. "The Nature of Belief Systems in Mass Publics." Pp. 206-61 in *Ideology and Discontent*, edited by David E. Apter. New York: Free Press.

Fisher, Burton R., Charles A. Metzner, and Benjamin J. Darsky. 1951. "Public Response to Peacetime Uses of Atomic Energy." University of Michigan, Survey Research Center.

Freudenberg, William R., and Rodney K. Baxter. 1983. "Public Attitudes toward Local Nuclear Power Plants: A Reassessment." Paper presented at the annual meetings of the American Sociological Association, Detroit.

³³ Our personal choice is for a methodology that we call "peer group conversations." We construct a group by sampling individuals and bringing the contact person together with three or four friends for a discussion of nuclear power and other issues. From analyzing such conversations, we hope to be able to address issues that surveys on nuclear power attitudes leave unresolved—issues concerning which media packages are used in what ways and how people negotiate meaning on the issue.

Nuclear Power

Fuller, John G. 1975. *We Almost Lost Detroit*. New York: Crowell.

Gamson, William A. 1988. "Political Discourse and Collective Action." Pp. 219-44 in *From Structure to Action: Social Movement Participation across Cultures*, edited by Bert Klandermans, Hanspeter Kriesi, and Sidney Tarrow. Greenwich, Conn.: JAI.

Gamson, William A., and Kathryn E. Lasch. 1983. "The Political Culture of Social Welfare Policy." Pp. 397-415 in *Evaluating the Welfare State: Social and Political Perspectives*, edited by S. E. Spiro and E. Yuchtman-Yaar. New York: Academic.

Gamson, William A., and Andre Modigliani. 1987. "The Changing Culture of Affirmative Action." Pp. 137-77 in *Research in Political Sociology*, vol. 3. Edited by Richard D. Braungart. Greenwich, Conn.: JAI.

Gitlin, Todd. 1980. *The Whole World Is Watching*. Berkeley and Los Angeles: University of California Press.

Gurevitch, Michael, and Mark R. Levy, eds. 1985. *Mass Communication Review Yearbook*, vol. 5. Beverly Hills, Calif.: Sage.

Halberstam, David. 1979. *The Powers That Be*. New York: Knopf.

Hodge, Bob, and Alan Mansfield. 1985. "Nothing Left to Laugh At: Humor as a Tactic of Resistance." Pp. 197-211 in *Language and the Nuclear Arms Debate: Nukespeak Today*, edited by Paul Chilton. London: Pinter.

Lovins, Amory. 1977. *Soft Energy Paths*. New York: Harper & Row.

McCracken, Samuel. 1977. "The War against the Atom." *Commentary* 64:33-47.

_____. 1979. "The Harrisburg Syndrome." *Commentary* 67:27-39.

Macdonald, Dwight. 1945. "The Bomb." *Politics*.

Mazur, Allan. 1981. *The Dynamics of Technical Controversies*. Washington, D.C.: Communications.

_____. 1988. "Mass Media Effects on Public Opinion about Nuclear Power Plants." Unpublished manuscript. Syracuse University, Department of Sociology.

Media Institute. 1979. *Television Evening News Covers Nuclear Energy: A Ten Year Perspective*. Washington, D.C.: Media Institute.

Mitchell, Robert C. 1980. "Polling on Nuclear Power: A Critique of the Polls after Three Mile Island." Pp. 66-98 in *Polling on the Issues*, edited by A. H. Cantril. Washington, D.C.: Seven Locks.

Nealey, Stanley M., Barbara D. Melber, and William L. Rankin. 1983. *Public Opinion and Nuclear Energy*. Lexington, Mass.: Heath.

Nelkin, Dorothy. 1981. "Nuclear Power as a Feminist Issue." *Environment* 23:14-20, 38-39.

Nimmo, Dan D., and James E. Combs. 1985. *Nightly Horrors: Crisis Coverage in Television Network News*. Knoxville: University of Tennessee Press.

Nisbet, Robert. 1979. "The Rape of Progress." *Public Opinion* 2:2-6, 55.

Sign, Leon V. 1973. *Reporters and Officials*. Lexington, Mass.: Heath.

Smelser, Neil J. 1963. *Theory of Collective Behavior*. New York: Free Press.

Snow, David A., and Robert D. Benford. 1988. "Ideology, Frame Resonance, and Participant Mobilization." Pp. 197-217 in *From Structure to Action: Social Movement Participation across Cultures*, edited by Bert Klandermans, Hanspeter Kriesi, and Sidney Tarrow. Greenwich, Conn.: JAI.

Swidler, Ann. 1986. "Culture in Action: Symbols and Strategies." *American Sociological Review* 51:273-86.

Tuchman, Gaye. 1974. *The TV Establishment*. Englewood Cliffs, N.J.: Prentice-Hall.

Waymack, W. W. 1947. "A Letter to Judith and Dickie." *National Education Association Journal*.

Williams, Robin M., Jr. 1960. *American Society*. New York: Knopf.

Structure after 50 Years: The Anatomy of a Charter¹

Charles Camic

University of Wisconsin—Madison

In the half century since its publication, *The Structure of Social Action* has emerged as one of the classics of the sociological tradition. At the present time, however, there is scarcely any agreement about the status of the book's argument among all those who still appeal to the volume. After 50 years, the vast scholarship generated by *Structure* is in disarray, with separate literatures existing for different aspects of the book and controversies present in all these literatures. This paper examines each major aspect of *Structure*: its (1) sociohistorical context, (2) writing style, (3) methodological argument, (4) account of the history of social theory, (5) analysis of action, (6) view of the social world, (7) perspective on the actor, (8) treatment of the problem of order, and (9) approach to voluntarism. The paper argues that, when *Structure* is embedded in the socio-intellectual context where it was produced, and is interpreted as a "charter" intended to defend the science of sociology against forces such as behaviorism and neoclassicism, the separate features of its argument come together to allow the resolution of many of the controversies that surround the book and to facilitate the integration of much of the previous scholarship. This interpretation locates the achievement and the critical limitations of *Structure* in Talcott Parsons's tendency to universalize the particular ideas that served to advance the immediate aims of his charter for sociology.

"817 arid pages" was Pitirim Sorokin's (1966, p. 495) short verdict on the undertaking, which has provoked similar responses from several generations of potential readers. But neither Sorokin nor the vast majority of

¹ I would like to thank Bernard Barber, Warren Hagstrom, Robert Alun Jones, Donald Levine, Steven Lybrand, and Erik Wright for their help and advice on this paper. I would also like to thank Jeffrey Alexander for making available to me the letter from Talcott Parsons cited in Section IV. Parsons's manuscript, "Prolegomena to a Theory of Social Institutions," is quoted by permission of the Harvard University Archives. Support for this research was provided by the Graduate School Research Committee of the University of Wisconsin—Madison. Requests for reprints should be sent to Charles Camic, Department of Sociology, University of Wisconsin, Madison, Wisconsin 53706.

others concerned with sociological theory over the past 50 years really dispensed so easily with the book. On the contrary, since its issue, in a small print run, late in 1937, *The Structure of Social Action* has established itself as one of the great "watershed[s] in the development of American sociology in general and sociological theory in particular" and as the virtual baseline for "the modern period in sociology" (Coser 1977, p. 562; Gouldner 1970, p. 61; see also Alexander 1987a, 1987b; Barber 1987; Gerstein 1979; Scott 1974; Shils 1961).² No sociological work from the period has been so widely discussed, and few theoretical developments in the discipline over the past half century have been launched without an engagement with Talcott Parsons's towering first book.

To date, however, this widespread discussion and engagement has produced surprisingly little in the way of a broader appreciation of where sociology stands with regard to the argument of *Structure*. The tendency, rather, has been for different theorists, scholars, and commentators (generally unaware of one another's work) to confront the book in piecemeal fashion and put forth discrepant interpretative conclusions, which then serve as the point of departure for sundry new theoretical projects, divergent analyses of the course of Parsons's intellectual career, and so on.³ It is the purpose of the present article to propose a *sociological and critical interpretation* that integrates the major features of *The Structure of Social Action* as well as the principal results of the large, fragmented scholarship spawned by the book.

As Lukes (1973, p. 1) has suggested, to attempt an analysis that is *sociological and critical* is to pursue divergent objectives that are nevertheless interdependent. A claim of the following research is that *Structure* cannot be adequately interpreted without a sociological understanding of the sociointellectual setting where the book was produced and of Parsons's social location and experiences in that setting. To achieve this understanding, the analysis takes into account not only the text of *Structure* but also Parsons's other writings before and more or less coincident with the book's appearance, his later autobiographical statements regarding his early career, and the work and situation of some of his earlier American contemporaries. In this sense, the study is an effort to apply to Parsons's thought the analytical strategy advocated by certain recent soci-

² In furnishing references, this paper adopts two conventions: (1) it supplies only original publication dates for cited materials, reserving fuller information for the References; (2) it designates *The Structure of Social Action* (Parsons 1937a) simply as *S*.

³ This is true even in the recent papers on *Structure*'s fiftieth anniversary by Alexander (1987a), Garfinkel (1987), and Turner (1987), though, for the most part, these papers concentrate more on the book's eventual influence than on its argument per se (see also Holmwood 1983; Sciulli and Gerstein 1985).

American Journal of Sociology

ologists of ideas (for reviews of this work, see Jones 1983, 1986) and to argue, by means of an example, the case for extending that approach. Somewhat paradoxically, though, the better one's sociohistorical understanding of *Structure*, the better equipped one is to offer a critical assessment of its present value: to discern, for example, whether the book contains intellectual resources that might dissolve our own preconceptions, or whether, in contrast, it holds its greatest present-day utility only when its original meaning is benignly overlooked. *Structure* is not, in my opinion, a pure example of either of these extreme alternatives. For reasons that will emerge, however, I do think that the book on the whole veers more in the latter than the former direction, and that contemporary sociologists might do well finally to recognize this possibility.

I

For all the attention *Structure* has received from sociologists, relatively few have sought to understand the book sociologically. Among those who have, the preferred procedure has been to adopt the tactics of the sociology of knowledge and appeal either to conspicuous aspects of Parsons's social background or to the macrohistorical climate of the 1920s and 1930s. Creelan (1978), for example, regards Parsons's work as a coming to terms with his early Congregationalist upbringing (see also Wearne 1983), while Gouldner has interpreted *Structure* in part as an alarmed reaction to the "mass meetings, marches, [and] demonstrations" of the Depression years (1970, p. 146; see also Gouldner 1979, p. 299; Therborn 1976). Putting both factors together, Alexander has more recently proposed that Parsons can "justly be discussed as [representing] the Protestant, moralistic, and middle-class 'protectionist' response to the development of capitalist society" (1983b, p. 385; see also Alexander 1987b, pp. 22–28; Buxton 1985; DiTomaso 1982; Vidich and Lyman 1985).

This approach brings out certain bona fide features of the case. Writing to Vogelin in the 1940s, Parsons himself noted a link between his scientific stance in *Structure* and his Calvinist heritage (see Wearne 1983, p. 6; also Johnson 1975, p. 125), and his unpublished papers from the period of working on the book demonstrate concern not only with the "many symptoms of our present economic situation" but also with "liv[ing] through the era of the great war, of communist and fascist revolutions, of oil scandals and Kruger debacles" (quoted in Buxton 1985, pp. 21, 282). It was, he later recalled, the draw of such developments, and especially "enthousias[m] for the Russian Revolution and for the rise of the British Labour Party," that first diverted him "from [an] inclination to biology and/or medicine, into the social sciences" (1971, p. 233; 1976a, p. 176). And *Structure* itself is plainly attuned to religious nuances and world

events, advocating a method that “points beyond the realm of the empirical [to] vindicat[e] the partisans of religion” and glancing outward to the Depression, Mussolini, Nazi methods of opinion control, and so forth (*S*, pp. 237 n. 1, 283 n. 4, 324 n. 2, 395 n. 1, 427–28). The notion that *Structure* “turned resolutely from any concern with the contemporary economic and political crisis” (Bottomore 1969, p. 34) is hard to sustain. But it is equally difficult to attribute to the book a defined practical program. A staunch New Deal liberal (Parsons 1961, p. 350), Parsons decried no political philosophy so much as liberalism—faulting it for neglecting “aristocratic” reservations about “progress in th[e] liberal sense” and for overlooking prospects of “a collectivist state where the . . . process of production [would be] in the hands of a . . . centralized body working in the general interests” (*S*, pp. 152–53, 292–93).⁴

At all events, instructive though they have been, standard sociology-of-knowledge approaches to *Structure* exhibit a common limitation. They center on factors whose level of generality is far removed from that of the book itself. Protestant middle-class affiliations and awareness of the dramatic events of the era were characteristic of large portions of the population in general, and the academic ranks in particular, during the 1920s and 1930s and are, as such, too commonplace to render so comparatively uncommon a “response” as *Structure* sociologically intelligible. Such factors may work for carefully selected features of the book but tend to leave the project as a whole elusive. It is otherwise, however, when attention shifts to the specific sociointellectual setting where the young Parsons was situated. Parsons said as much himself toward the end of his life, observing that he “was a ‘child of [his] age’ and, in working on [*Structure*], thought in terms of the intellectual environment of the time” (1978, p. 1351; also 1976b, p. 364). Indeed, concerned though the early Parsons was with broader world affairs, the main focus of nearly all his writings lay in certain of the intense controversies that were then raging across the American academic scene. Here, note can be taken of only a few aspects of this complex scene.⁵

Of greatest importance is that *Structure* took shape during the seedtime of the modern academic landscape—the period, running from the last quarter of the 19th century to the Second World War, of the expansion of American higher education from its modest origins into a nationwide network of colleges and universities, which were internally divided into

⁴ For fuller treatments of Parsons’s political views, see Alexander (1983b), Apter (1987), Buxton (1985), Foss (1963), Gouldner (1970), Holton and Turner (1986, pp. 209–34), Lipset and Ladd (1972), and Mayhew (1982, esp. 1984).

⁵ In addition to primary source material, the account in the next four paragraphs draws heavily on Camfield (1973), Cravens (1978), Curti (1980), Hinkle (1963, 1980), Oberschall (1972), Parrish (1967), Ross (1979), and Samuelson (1981).

American Journal of Sociology

departments representing those fields that successfully established themselves amid fierce interdisciplinary struggles for the academy's finite material and ideal rewards. The most consequential of these struggles involved the distinguished biological sciences and the emerging social sciences and revolved around the claim (by the former) that recent experimental discoveries in biology—ramifying as they did to the foundations of life, both individual and collective—finally “unlocked the secrets” of the social world and thereby obviated the need for separate social sciences (see Cravens 1978, p. 11). In an age open to “the application of biological science to social problems” (Cravens 1978, p. 17), this was a compelling idea and one made still more convincing following the institutionalization, in the early decades of this century, of the discipline of psychology as one in the family of biophysiological sciences. For whether its emphasis fell (as in the pre-World War I period) on heredity and instinct or (as in the postwar era) on the role of the physical environment, the clear message of psychology was that its natural-scientific researches held unique promise of resolving everything from “the basic issues of the origin and development of mind [to] the social problems of a disorderly industrial society” (Cravens 1978, p. 57). And this was a message powerfully reinforced, from the mid-1920s onward, with the spread through psychology of the “behaviorism” of John Watson, which aggressively reduced the study of social institutions to the biophysiological study of human beings, deterministically conceived as “nothing [more than] reactive animal[s], without soul [and] consciousness,” without “‘choice’ and ‘purpose’” (Cravens and Burnham 1971, p. 646; Fearing 1930, p. 311; see also Allport 1927).

Social scientists reacted to this onslaught in different ways. Too factional to mount a unified counterattack, they took positions corresponding to how their own fields stood in the interdisciplinary competition. Anthropology, for example, possibly the most weakly established of the emerging disciplines, initiated the boldest response. It carved out the idea of “culture” (conceived as values, attitudes, and collective symbols) and insisted—most militantly perhaps in the Boasian tradition of Lowie, Kroeber, and others—that human thought and action “are determined by factors arising not from the [psychobiological] characteristics of the . . . actor but from the cultural conditions . . . of his group” (Lowie 1924, p. 277; see also Cravens 1978, pp. 89–120). At the opposite extreme, economics, which attained lofty academic status even earlier than some of the biophysiological sciences, long adhered to its orthodox neoclassical heritage, unruffled by the challenge of biology, with which it actually united in opposing the establishment of other social sciences. By the interwar period, however, even orthodox theorists like Taussig, reacting to institutionalist movement inside economics, encouraged accommoda-

tion not only to the traditional fields of “history, politics, [and] ethics” but also to the new sciences of “psychology [and] biology” (1924, p. 13).

Typical of the age, Taussig’s list makes no mention of sociology, a reflection of the reigning assumption that the less said about it, the better. Indeed, although courses labeled as sociology had (thanks to part-time instructors in nonsociology departments) long since appeared in American colleges in response to the demands of reformers and others for vocational training, acceptance of sociology as a worthwhile intellectual discipline—and independent department—was rare down to the 1930s (see Oberschall 1972; Ross 1979). The assault by behaviorist psychologists on the social-scientific domain, combined with the resistance of those like the neoclassical economists, hit many would-be sociologists with particular force and imbued them with “the need to define sociology in university culture, to differentiate it from other disciplines, to win recognition for it [by delineating] its methods, assumptions and body of data, [and] to establish [its] achievements in research and theory” (Cravens 1978, p. 140).

To meet this need, leading sociologists like Cooley, Ross, Thomas, Znaniecki, Ellwood, Park, Faris, and MacIver worked out, in the two decades before *Structure*, a position similar to that developed by the anthropologists and increasingly common, too, among antibiologistic social philosophers (Dewey, Mead) and gestalt psychologists (Koffka, Köhler). They affirmed that humans in society are cultural beings rather than reactive animals: “conscious agents” and “active subjects” (in Znaniecki’s terms [1934, p. 131]), affected by heredity and environment, but neither determined thereby nor understandable without “notion[s] of voluntaristic action [and] articulated purpose”—that is, without an appeal to human attitudes, values, and symbols and to such behavioristically suspect phenomena as “subjectivity, . . . ends, means, reflection and choice, [and] rules” (Janowitz 1975, p. 86; Hinkle 1963, p. 714; see also Cravens and Burnham 1971, pp. 649–54; Curti 1980, pp. 239–46; Kolb 1957, p. 106).⁶ The polemical climate of the period did not encourage precise specification of these concepts and their interconnections. More urgent to contemporary sociologists, inspired by the example of the established sciences, was to advance from their premises to concrete empirical research, and this is what they undertook in a great diversity of areas. Over the long run, such efforts markedly benefited the cause of sociology. In the short run, however, they left the field near the bottom of the academic hierarchy, so much so that as late as 1939 the *Saturday Review*

⁶ This point of view was not, to be sure, upheld unanimously. There was, at the time, a group of sociologists whose response to behaviorism was to propose a sociology that could be defended by the behaviorists’ objectivist criteria for science. For an analysis of the experiences that underlay this group’s distinctive response, see Bannister (1987).

American Journal of Sociology

of *Literature* could print as its lead “What’s the Matter with Sociology?” an article by Harvard historian Crane Brinton jeering the “would-be science”—“the poor thing [that] has been ailing [since] Comte”—as “the pariah subject,” whose “trivial content,” discrepant research findings, and lack of any “solid and established central core” placed it more on a par with “alchemy” than with either the natural sciences or economics (1939, pp. 3–4, 14).

The Structure of Social Action is a late and sweeping effort to defend the opposite position in the ongoing interdisciplinary controversies. Parsons was mortified that Brinton did not appreciate this (see his memo to Henderson in Buxton 1985, pp. 284–85). When he later characterized himself as a “child of his age,” one of the things Parsons especially emphasized was his deep involvement “in the ‘war of independence’ of the social sciences vis-à-vis the biological” and in the struggle against the era’s “rampant . . . behaviorism” (1959a, pp. 625–26; 1970, p. 830; see also Bourricaud 1977, p. 11). His “polemical foil” during this period, he often insisted, very much “lay in radical behavioristic theories . . . stemming from John B. Watson” (1974, p. 56; also S, pp. 77, 115–16, 190, 356; 1959b, p. 8). But the early Parsons was also convinced that a general victory for the social sciences in their war against the biological sciences would not suffice to elevate the discipline of sociology from its status as “a ‘poor relation’ . . . in the orbit of [the] social sciences” to a rightful place alongside the eminent field of neoclassical economics (1936a, p. 372; also S, p. 598; 1969b, p. xiv). For this reason, he felt it necessary to concentrate on “the problem of the relation between economic theory [and] sociological theory” and, within the context of his larger defense of the social sciences, to set this as “the focal problem” of *Structure* (1975b, pp. 666, 669; similar statements are to be found in many of Parsons’s works).

Considering his own social location and experiences, Parsons had ample cause to regard the biological sciences and economics with even more concern than did other sociologists of the age. For, however much Harvard University “insulated” him from the economic crisis of the 1930s (Gouldner 1970, pp. 169–78), it had the opposite effect with respect to the interdisciplinary struggles. A world leader in biology, physiological psychology, and neoclassical economics, the university lagged behind many peer institutions when it came to sociology, countenancing decades of opposition to the field’s independence—by some of the economists and thinkers of Brinton’s type—before establishing a tiny department of sociology in 1931 (see Church 1965; Sorokin 1963, pp. 240–43). Magnifying conditions on the American academic scene further was Parsons’s own career trajectory, which began with undergraduate training in the distinguished field of biology; moved from there to graduate work in

economics and an instructorship in Harvard's front-ranking department of economics, where promotion prospects were slim; and culminated, in the years when he was writing *Structure*, with a transfer to a tenuous position in the low-status sociology department. Under these circumstances, it is little wonder that, throughout *Structure* and the articles that preceded it, Parsons took challenges from the behaviorists, the economists, and others seriously and developed a deep interest in "firmly [establishing] sociology's credentials as a recognized form of social-scientific inquiry [and] clarifying its subject matter, method, and relations" with other disciplines (Buxton 1985, p. 76; also Levine 1980, pp. xxxviii–xli).

In approaching these issues, Parsons—understandably—tapped the available intellectual resources and, like many of his contemporaries, sought recourse in a perspective that, even as it conceded the role of heredity and environment, viewed human conduct as essentially voluntaristic and animated by common values, subjective purposes, ends, rules, and so on, that is, by the distinctively sociocultural phenomena that *Structure* terms "normative." The point is not that Parsons was particularly "influenced" by the writings of like-minded American sociologists, philosophers, and others—although he was impressed with the gestalt psychologists' argument for voluntarism, knew the work of Cooley and Znaniecki well, and had a good personal acquaintance with sociologists from Chicago and Columbia (see Parsons 1959b, p. 4; 1974, p. 56; Janowitz 1970, p. xi).⁷ If Parsons's themes echo with the ideas of others from the 1920s and 1930s, a more likely explanation is that he was struggling under conditions similar to theirs, against the same intellectual and institutional opponents, to accomplish the same disciplinary objectives. The early reviewers of *Structure*, who operated in the same universe of discourse, took clear note of this, recognizing the book as a contribution to the attack on behaviorism and economic theory and seeing in the emphasis on voluntaristic action, values, and the rest a means to justify "the independent existence of sociology" and "give it a more respectable position" (Bierstedt 1938b, p. 665; Kirkpatrick 1938, p. 588; also Catlin 1939, p. 265; Crawford 1938, p. 179; Huse, 1939, p. 129; Wirth 1939, pp. 403–4).⁸ Commentators since the 1950s, nonetheless,

⁷ The young Parsons also had general familiarity with trends in anthropology. He cites Lowie's writings, as well as those of Boas and Radcliffe-Brown, and studied at the London School of Economics with Malinowski, whose work he associated mainly with "one of the greatest accomplishments of anthropology [in] Malinowski's time: a clear distinction between man as a biological organism and man as the creator, bearer, and transmitter of culture" (Parsons 1957, p. 85).

⁸ Contrary to common opinion, *Structure* was (with the one exception noted below) prominently and generally favorably reviewed (see, in addition to the notices cited above, Bierstedt 1938a; Gettys 1939; House 1949; Pinney 1940; cf. Simpson 1938).

American Journal of Sociology

have generally lost sight of the book's intentions here—so completely that none of the most recent synoptic treatments of *Structure* so much as mentions its disciplinary aspirations (see Alexander 1987a; Garfinkel 1987; Turner 1987). In the past three decades, only a few scholars, focusing on particular aspects of the work, have observed the extent to which Parsons's views were shaped by his interdisciplinary travails (see Burger 1977, p. 326; Gouldner 1970, p. 18; Levine 1980, p. xxxviii–xli; 1985, pp. 119–22; Lockwood 1956, p. 134; Mayhew 1982, pp. 3–10; Mills 1959, p. 35). When this scholarship is brought together, however, it becomes clear that this shaping was not limited to particular aspects of *Structure*. It is evident in the whole enterprise.

Two qualifications are important here. First, the point of the preceding argument is to identify Parsons's disciplinary concerns and struggles as constitutive of the basic design and shape of *Structure*; it is not to suggest that everything woven into that design is reducible to this one foundational factor. Determined to succeed in a competition that was defeating those sociologists who put empirical research before the thorough treatment of "fundamentals" (Parsons 1937b, p. 13), Parsons was willing to embrace conceptual, interpretive, and historical problems with complex lives of their own—and to pursue them down long paths that attest more at times to the topics immediately at issue than to disciplinary considerations per se. Second, to understand *Structure* as a document in the interdisciplinary battles of the past is not to denigrate it as the product of the ulterior career interests of its author. An integral part of the reason Parsons and his contemporaries fought hard for autonomous positions for themselves and their prospective students in the universities was their desire to challenge the then powerful assumption that a science of the social world could be built with concepts like "reflexes, instincts, [and] economic interests" (Williams 1971, p. 124). Few would now deny that Parsons's labor here was on the side of the angels.

But there is a further dimension to this, one that serves, I would suggest, to draw together the fragmented scholarship on *Structure* and allow an integrated critical assessment of the current status of its argument. When one ranges over the extensive literature on the book while keeping its sociointellectual context in view, what emerges, on issue after major issue, is that Parsons adopted a position well suited to his objectives and circumstances but that he then represented that particular position as the natural gauge for sociological analysis in general, thereby converting his own reasonable historical choices into a universal touchstone for the present and future. It is thus that the concepts and categories, method, historical propositions, textual interpretations, theoretical framework, and empirical observations that sustain the important particular argument of *Structure* come to be projected by Parsons as the core of

the sociological enterprise as a whole, with other options (other observations, frames of reference, interpretations, etc.) either assimilated into these, marginalized, or occluded outright. This observation must be differentiated from Gerstein's remark that *Structure* "is admittedly . . . limited, [but] what finite analysis is not?" (1979, p. 38). For it is not that Parsons just so happened, like everyone else, to stop short of taking into account all relevant things. The point is that there is a definite pattern behind what the book did and did not encompass, that this pattern was in part keyed to the disciplinary struggles in which Parsons participated, and that Parsons represented the "finite analysis" that resulted as the exclusive foundation for sociology at large. It is for this last reason, I think, that the "normative" slant found in so much of *Structure* has occasioned such objection to the book—not the slant itself but its presentation as something more global.

In the lexicon of the social sciences, there is a well-known term for ideas that project the particular and local as the general and universal. In the tradition extending back to Marx and Engels, that term is, of course, "ideology"—but ideology in only one of its several meanings, most of which have little bearing on *Structure*. It is appropriate, therefore, to factor out of this complex of meanings "the tendency to present as a group's general condition and interest what defines and serves group members' particular goals under specific historical conditions" and to speak of a text exhibiting this tendency as a "charter." From this angle, instead of interpreting *Structure* (like those discussed above, such as Gouldner [1970] and Buxton [1985]) as the "ideology" of an already established socioeconomic or religious group, we may approach it as a charter, a charter for the still-emerging science that was the domain of the sociologist. To do this is not to deny that Parsons had political and quasi-religious reasons for finding a charter for sociology personally attractive and believing it would appeal to others as well.⁹

II

"It is unfortunate that it is so long and so abstruse in style," wrote Floyd House (1939, p. 130) in an early review of *Structure*, as he put forth a

⁹ Note should especially be taken of Parsons's conviction that developments like the rise of Nazism were "profoundly *moral* problems, demanding not just economic and political solutions," but treatment "at the level of general theory" by the sociologist (1969a, p. 60; 1971, p. 233). In a 1934 speech he stated: "My own preoccupation has been with some of the broadest and most general questions of sociological theory, not with the details of practical problems and programs. [But] the answers of general sociology to these questions, though far from being detailed and specific, are not without their concrete and even practical relevance" (quoted in Buxton 1985, p. 281).

American Journal of Sociology

view that has since become almost obligatory. Typically, however, commentators affirming this view have regarded the book's form more as an annoyance than as a revealing expression of its central purposes (cf. Gouldner 1970, pp. 199–209; see also Bierstedt 1981, pp. 389–442; Creelan 1978). The point requires brief elaboration.

Contrary to near-unanimous opinion, *Structure* is not, in fact, an unrelieved exercise in bad writing. That the text has its unwieldy and repetitive sections is obvious enough. Yet it also contains a good number of lean and vigorous passages and certain formulations so apt they are cited all over the field. To debate the literary character of *Structure* on a paragraph-by-paragraph basis, however, is to miss the powerful effect that the entire work produces because of what is perhaps its most striking formal element: its programmatic sweep. Difficult passages notwithstanding, the book carries one relentlessly along on an inquiry that is at once a treatise on scientific method, a defense of human voluntarism, a historical account of trends in Western social theory over three centuries, a statement of the analytical foundations of social theory, a study of the causes and solutions for the problem of social order—and, through it all, an attempt to classify the various sciences and specify their interrelations. This side of the *Cours de philosophie positive*, there is probably nothing like it, and by the volume's end, with everything approximately tied together, the reader is almost too outflanked to fend off the climax: the proclamation of sociology as an autonomous science, with all the rights and privileges pertaining thereto. This is the sense in which *Structure* resembles what Anglo-American law terms a "charter of incorporation": a public document designed to constitute a formal association or corporate group by delineating its distinct purposes, intended operating procedures, available resources, historical background, and future objectives and so claiming for it a fixed identity and the various rights and privileges attendant on separate corporate status (see standard legal dictionaries and, for a sociological treatment, Meyer [1970]). Blanket criticism of *Structure* on the grounds of form simply overlooks the degree to which the book is artfully constructed to bring together and arrange architectonically the various components of a Magna Carta for sociology—and to do so within a framework invitingly presented nonetheless, as Gouldner (1970, p. 177) pointed out, as still in need of further collaborative development.

To say this is not to deny, however, that *Structure* is, like many an important legal charter, forged with an opaque vocabulary. With ample justification, every major term in the book—positivism, utilitarianism, empiricism, idealism, voluntarism, rationality, subjective, cognitive, normative—has come under scholarly criticism for its idiosyncratic us-

age, loose definition, and shifting denotation.¹⁰ But such difficulties should not be attributed merely to Parsons's peculiarities as a writer. The problematic terminology is very much bound up with the book's mission as a charter. This is so in two ways.

First, as a result of its programmatic sweep, *Structure*'s terms are strained to accomplish considerable bridging. By employing the same concepts now on this front, now on that, now on another, Parsons imparts the appearance of unity to his multipronged campaign—though at a cost. "Positivism," to take a single example, is used simultaneously to designate an analytical theory focusing solely on the cognitive aspects of an actor's orientation, a methodological approach seeking to cast the social sciences in the model of the natural sciences, and a historical moment in post-Reformation European thought (S, pp. 61, 87, 180–81). These meanings are, in addition, freely projected onto one another, so that proponents of the positivist method are portrayed as supporters of the positivist analytical theory and heirs to the long positivist tradition, which is seen as representing both the theory and the method (see Alexander 1983b, pp. 16, 273, 439–40; Ford 1974; Lundberg 1956). This kind of playing on words entangles *Structure* in a thicket of artificial isomorphisms, dubious strawmen, and perplexing loose ends (see Barry 1970, p. 79; Scott 1963, p. 732; Sorokin 1966, pp. 406–7), thus requiring the reader to assent to associations that, however much they tie together the components of Parsons's charter, are unlike those customary in ordinary academic discourse, whether today or 50 years ago (cf. Pinney 1940, p. 184).

But the terminological opacity of *Structure* has a second, more basic source, one affecting present-day readers in contradistinction to Parsons's contemporaries. Contemporaries understood that the book was an intervention into ongoing interdisciplinary debates and that its concepts were often devices that, for all their internal murkiness, pointed outward to identifiable camps of polemical opinion. But, although Parsons frequently named these contemporary referents, modern interpreters still wrestle with *Structure*'s terms without first contextualizing them in now-forgotten strands of biologism, behaviorism, neoclassicism, and so on, and understanding that the code by which those terms are related to one another is itself firmly rooted in the dichotomy between the biological and

¹⁰ On "normative" and "voluntarism," see Sections V and IX; on "utilitarianism," Barry (1970, pp. 77–79), Scott (1962, p. 61); on "empiricism," Bierstedt (1981, p. 395), Giddens (1982, pp. 79–81), Savage (1981, p. 25); on "idealism," Clarke (1982, p. 3); on "rationality" and "subjective," Schutz (1940–41); on "cognitive," Warner (1978); and on "positivism," Adriaansens (1980, pp. 34–36), Menzies (1976, pp. 32–33), Rocher (1975, p. 26), and the sources cited in the following paragraph.

American Journal of Sociology

the cultural that defined the academic scene of the 1920s and 1930s. Parsons was forthright about this, writing of *Structure* that “the great dichotomy of this study” is that of “heredity and environment”—“in the . . . sense of biological theory”—on the one hand and the normative or “value factor,” anchored in “the element[s] of culture,” in “religious, metaphysical and ethical systems of ideas” and the like, on the other hand (S, pp. 64, 426, 449, 464; 1934a, p. 231).

As a participant in the disputes of that time, Parsons was never neutral about these two sets of elements. His tendency, rather, was to load whatever was useful for his charter onto the “value factor,” while associating what was objectionable with its opposite. And, indeed, some of the terminological mist over *Structure* dissolves when one appreciates that its “definitions” often work less to assign precise meanings than to associate key concepts with one *or* the other side of the book’s “great dichotomy.” It is this dichotomy that underlies, too, Parsons’s proclivity to “slip . . . into an either/or approach” on strategic questions (Alexander 1987b, p. 29), most conspicuously the “forced analytic choice” between “random, amoral, self-interested, disintegrative, individualistic chaos” and “normatively structured, value-oriented, integrative, social order” (paraphrase of Warner 1978, pp. 1324–26). It is true that, in the context of the time, this choice appeared more natural than “forced” and that, in embracing its simple logic, Parsons sharpened the claims on which his charter is based. But it is also the case, as will become clear, that a logic of this kind is severely constrained both in its own terms and with respect to elements neither encompassed in the dichotomy nor accommodated when its sides are seen—as *Structure* does sometimes see them—as the opposite poles of a single axis. The assumption that this particular logic is the logic of social theory in general runs, nonetheless, throughout Parsons’s book.

III

Parsons emphasizes in *Structure* that the value factor, or normative element, in his dichotomy is an “independent factor” not “derivative of [the] biological factor” (S, pp. 44, 168, 260 n. 3). To deny this independence is, in his view, to render value elements “epiphenomena,” thereby undermining the rationale for an autonomous sociology (1935a, p. 286). He seeks, consequently, to guarantee the independence of the normative factor, doing so by maintaining that the cultural elements that anchor this factor “point . . . beyond the realm of the empirical” to “nonempirical aspects of [the] universe”—“hanging in the air” (S, pp. 424–28; 1934b, p. 531). In upholding this position, however, Parsons does not, as commentators have sometimes suggested, accept a “metaphysical dualism” that would set matter and mind apart and require value elements to be ana-

lyzed in ways "methodologically independent of the sciences of nature" (Scott 1963, pp. 723–24; see also Friedrichs 1970, p. 13; Savage 1981, pp. 92, 109–15). His book expressly rejects this kind of "radical dualism," asserting that normative factors "participate in empirical reality" (even as they point beyond it) and, while distinct from natural-scientific phenomena "on a substantive level," remain open to treatment by the same "method"—that is, by the same analytical and explanatory procedures—as fields like biology, chemistry, and physics (*S*, pp. 448, 474, 595–98). The "independence" of the normative dimension does not mean, in short, that to study this element is to compromise the method that has established these other disciplines as full-fledged sciences.

Structure's defense of the "unity of method" has been widely recognized in recent years. Scholars remain divided, however, on the character of this defense. To some, the book embodies a narrow kind of empiricist and positivist philosophy of science (see Hall 1984; Johnson, Dandeker, and Ashworth 1984; Keat and Urry 1975; Rossi 1981; Taylor 1979; Wilson 1983)—with "empiricist" here roughly meaning the claim that scientific knowledge originates chiefly from systematic observation or sensory experience, and "positivist" the claim that the natural and social sciences are predicated on a common method that seeks to discover constant, uniform laws (Giddens 1974, pp. 3–4).¹¹ To other interpreters, *Structure* represents a revolutionary shift away from America's positivist-empiricist assumptions to the "postpositivist," "antiempiricist" idea that scientific knowledge is inherently premised on theoretical or analytical abstraction (see Adriaansens 1980; Alexander 1983b; Barber 1986; Bershady 1973, 1974; Bourricaud 1977; Hamilton 1983, 1985; Heritage 1984; Johnson 1981; Lassman 1980; Lidz 1986; Martel 1971, 1979; Münch 1981, 1982, 1987; Procter 1978; Rocher 1975; Savage 1981; Schwanenberg 1971, 1976). When the method recommended in the book is viewed in connection with its larger objectives, however, it becomes clear that Parsons actually adopted a midposition between what these contrary readings suggest.

In formulating his method, Parsons did quickly dismiss what he called—speaking broadly, rather than in accord with a philosophy-of-science definition—the "empiricist" orientation of his contemporaries in sociology. As previously noted, his opinion was that the cause of sociology was endangered by prematurely rushing into diverse empirical research activities. What he engaged more fully, however, was the methodological position of a number of contemporary neoclassical economists whose

¹¹ As noted above, *Structure's* own conception of positivism is a broader one; the same holds for its view of empiricism (see *S*, pp. 6–10, 59, 69–70). The definitions used here are for the analysis of methodological issues only.

work he had encountered while still an economics instructor; this engagement proved an extremely important one owing to the distinctive conception of science proposed by neoclassicists, and summarized perhaps best in the writings of Parsons's friend Frank Knight. Significantly, this conception had very traditional foundations: economics, wrote Knight, is a "true, and even exact science, which reaches laws as universal as those of mathematics and mechanics" and does so accepting the "empiricist [premise] that all general truths . . . are ultimately inductions from experience" (Knight 1921, p. 8 n.; 1924, pp. 234, 256–57). But, once laid down, these foundations were defended in up-to-date ways. According to Knight, "there is . . . little if any use for induction in the Baconian sense of an exhaustive collection and collation of facts"; "there [cannot] be facts apart from theories" and "observation [operates] relative to [an] interest" supplied by the scientist, whose "thought is saturated with purpose and concepts, emotion and metaphysical entities," and whose theories are necessarily "abstract," "select[ing] some generally important aspect of phenomena and deal[ing] with that to the exclusion of other aspects" or elements (Knight 1921, pp. 3, 7 n. 1; 1925, pp. 95, 97n.; 1928, p. 141; 1940, p. 462). Economic theory itself concentrates on "the *economic* aspect of human behavior" and constructs from the interplay of deduction and induction, of theoretical principles and "detailed factual study," an analytical "structure [of] ever-growing . . . intricacy," yet, in its selective way, "descriptive of reality" (Knight 1921, pp. 6–7; 1924, p. 258; 1928, p. 141; 1940, p. 462).

Parsons encountered similar views elsewhere: in the works of Pareto and Marshall; in the writings of Harvard economists like Young, Carver, Taussig, Taylor, and Schumpeter; and in the teachings of Harvard's reigning authorities on science, L. J. Henderson and Alfred North Whitehead—the last five often named by Parsons as particularly influential in his methodological thinking (e.g., *S*, pp. xxii–xxiii; 1959a, p. 625; 1959b, pp. 6–7; 1970, pp. 827–30, 873–74; see Barber 1970; Camic 1987; Devereux 1961; Lidz 1986; Münch 1981; Vidich and Lyman 1985; Wearne 1982, 1983). Why he opened himself to this line of influence is something he also disclosed. In his view, "the great . . . source of the preeminence of economics" directly derives from its method, which is "closest in theoretical form to the natural science model" of "generalized [analytical] theory." Were sociology in possession of an analogous approach, the field would then lie "on the same level as economic theory" and "find a place by its side" (1936a, p. 372; *S*, pp. 301, 598, 623, 772).

Given Parsons's programmatic mission, "it seemed," therefore, "to be the moment to search for an equivalent in sociology" of the economists' method (1959b, p. 620). This equivalent, modified to fit the normative element rather than the "economic aspect" that occupied the neoclassi-

cists, is what appears in *Structure*. At the center of its methodological argument is the then-familiar idea that “a given [scientific] field can never [grasp] ‘all the facts,’ ” its focus being not “the full totality of knowable facts, even about the concrete phenomena studied, but . . . certain selected elements of the latter,” that is, certain “analytical elements” (*S*, pp. 34, 582, 600). What the selected elements will be is contingent on “the direction of scientific interest,” especially as embodied in the theoretical framework of a discipline at a given time. Using the terminology of Henderson, Parsons held that “all empirical observation is ‘in terms of a conceptual scheme’ ” that directs scientists “to those aspects of concrete phenomena which are important to the theoretical system” in question (*S*, pp. 28, 597, 600). In light of this, he strongly affirmed “that the development of . . . scientific knowledge [is] a matter of the reciprocal interaction of new factual insights and knowledge, on the one hand, with changes in the theoretical system, on the other” (*S*, p. 11; see Barber 1986).

Parsons’s commitment to this position vitiates the claim that *Structure* defends a narrow type of positivist-empiricist methodology. But interpreters who recognize this point overshoot the mark in viewing the book as a revolutionary move to postpositivism and antiempiricism. Even as he emphasized the role of theoretical abstraction, Parsons affirmed the (positivist) idea that every analytical science—whether biology, economics, or sociology—aims to render “the facts of empirical experience” understandable in terms of “uniform laws” (*S*, pp. 69, 598, 622). These laws are, to be sure, “analytical laws” that pertain only to particular elements of reality, but they specify nevertheless “the constant mode of relationship” among the values of such elements and thus afford a realistic “grasp” of these features of the “external world”—hence the label “analytical realism” that Parsons applies to his approach (*S*, pp. 582, 622, 730). Furthermore, while insisting that scientific knowledge emerges on a day-to-day basis from the interaction of the theoretical and the factual, *Structure* endorses the (empiricist) thesis that theoretical frameworks themselves are ultimately “the product . . . of observation, reasoning, and verification, starting with the facts and continually returning to the facts”—and seeking a final point where “elements which do not lie in the facts themselves . . . are eliminated” (*S*, pp. xxii, 369). The charge, by those like Homans (1962, p. 46), Mills (1959, p. 33), and Turner (1987) that Parsons gave abstract categories precedence over factual observation in the development of theory runs completely counter to the position formally taken in *Structure*.¹²

¹² It is significant that, even as he worked on *Structure*, Parsons was involved in direct observational research on the medical profession and in classroom teaching that had a strong empirical component (see Barber 1985, 1986, 1987).

American Journal of Sociology

That Parsons embraced an “equivalent” of the method of the economists does not deny that his views signify an important break with those of his contemporaries in sociology (but see Turner 1986). Appreciation of this achievement should not obscure its limitations, however. Eager to endow sociology with the method that conferred high status to economics, Parsons adopts an approach in *Structure* that fails to accommodate adequately two long-standing sociological concerns. The first of these is subjectivity and meaning. Lidz makes the point well: “In building his . . . methodological foundations [to accord with the natural sciences, Parsons] seems not to have thought it necessary to examine the problems of *Verstehen*”; though action and meaning are his substantive focus, his method is not “carefully adjusted to the phenomena of action”—to the fact that “epistemological relations are changed where the phenomena consist of meanings, expectations, emotions, norms, and beliefs . . . and where the observer must gain knowledge by empathically reconstituting the situations and outlooks of other actors” (1986, pp. 154–55; see also Hall 1984; Heritage 1984; Schutz 1940–41; and the articles in Wolf 1980).

A second, related problem is that of historical variety. Throughout *Structure*, Parsons insists, following the neoclassicists, that explaining particular concrete phenomena is a task that falls *not* to his own science but to the discipline of history; with the neoclassicists and in opposition to those like Weber, he holds, further, that to explain a given historical configuration, the historian has simply to “analy[ze the] historical individual into elements, each of which [can] be subsumed under a general law,” as formulated by the analytical sciences (S, pp. 628, 760). In lieu of a historicist concern with the distinct differences among social configurations, Parsons therefore proposes—solely—the analysis of the variation of the values of the same, transhistorical analytical elements (S, pp. 618–24; see Camic 1987; Robertson 1969; Zaret 1980). In so doing, he elevates the specific method most useful for chartering sociology in the 1930s as *the* method of the field; alternative approaches are foreclosed.

IV

Despite his attraction to the method of economics, Parsons had serious substantive objections to the field, objections fueled by the economists’ opposition to the establishment of sociology. Realizing this opposition would not be overcome simply by setting his own opinions against the classical and neoclassical traditions, however, Parsons appeals in *Structure* to another heritage: the more modern legacy of certain distinguished European thinkers who provided “ancestral authority for his own [vision]

of sociology" (Cohen, Hazelrigg, and Pope 1975a, p. 229; also Clarke 1982, p. 2).

While Parsons was not alone in turning from orthodox economics to other currents in European thought, what is distinctive is the breadth of his intellectual-historical argument and the intricacy of its analytical apparatus. Veblen (1898, 1899–1900), Mitchell (1910a, 1910b), and other institutionalist economists, whose writings were well known to Parsons, consistently attacked neoclassicism for its atomistic individualism and assumptions about rationality and the givenness of wants, but Parsons melded such criticisms together in the form of the "utilitarian dilemma." He characterized utilitarian thought—a category embracing the neoclassicists of his time and their predecessors back to Spencer and to Locke and Hobbes—as committed, by its emphasis on rational efficiency at the expense of other criteria for selecting means to ends, to the position that "scientifically verifiable knowledge [is the actor's] only significant orienting medium in [an] action system" (S, pp. 60–61). According to Parsons, this position, combined with the utilitarians' treatment of actors as separate individuals pursuing given ends, entailed a conception of ends as "random in the statistical sense": a view unacceptable in its own terms and unable to explain adequately why social order, rather than "conflict," obtains in human societies (S, pp. 59–60, 89). The "dilemma" was, however, that so long as utilitarian thinkers adhered to "positivism" (i.e., so long as they focused solely on cognitive factors), their only option was either to accept these inadequacies or to patch them over by denying the randomness of ends or abandoning the efficiency standard with explanations stated "in terms of [the] nonsubjective conditions [of] heredity and environment," terms that eliminated what utilitarianism sought to recognize, "the active agency of the actor in the choice of ends" (S, pp. 64–67).

From this, Parsons concluded that the edifice of modern economic theory rested on a foundation marked, for nearly three centuries, by "an inherent instability" and "tendency [toward] breakdown" (S, pp. 69, 86). He maintained, however, that it was not required that this breakdown end in the biological categories of an extreme, radical positivism; in the late 19th and early 20th centuries, Alfred Marshall, Vilfredo Pareto, and Émile Durkheim all finally discovered that the lacunae of utilitarian thought (random ends and the problem of order) could be overcome, with no sacrifice of the actor's agency, if common values and other normative elements were incorporated into the analysis of human conduct. The three hereby advanced from the "positivistic theory" to the "voluntaristic theory of action," which emerged independently in the work of Max Weber and did so despite Weber's grounding in the "idealistic theory of action," which saw conduct as an "‘emanation’ of . . . normative factors"

American Journal of Sociology

and disregarded conditional obstacles and considerations of rationality and efficiency (*S*, pp. 82, 446).¹³ Parsons hailed this convergence of these “recent European writers” as a movement of thought unparalleled since the 16th century. Characteristically, however, he described the specifics of the “convergence thesis” differently at different places in *Structure*, with emphasis falling sometimes on the fact that Marshall, Pareto, Durkheim, and Weber all analyzed action through a congruent scheme of “structural categories” (see Sec. VI); at other times, on the recognition by the four of the importance of both normative elements and utilitarian elements (i.e., efficiency, instrumental rationality, or scientific knowledge); and, at still other times, on their incorporation of normative factors in addition to the “conditional” factors of heredity and environment (*S*, pp. 5, 451–70, 683, 698–719, 775).

The historical argument of *Structure* is the most famous interpretation ever offered of the development of the sociological tradition and has long been accepted, in varying degrees, by scholars in and out of the discipline (e.g., Alexander 1982a, 1983b, 1987a; Annan 1959; Atkinson 1972; Burrow 1966; Clarke 1982; Eisenstadt with Curelaru 1976; Gerstein 1979, 1983; Habermas 1981b; Hughes 1958; Seidman 1983; Shils 1961; Turner 1987). The more closely it has been examined, however, the more it has become clear, as Levine has written, that *Structure*’s “reconstruction of the sociological tradition must be judged erroneous on every fundamental point” (1980, p. xxv).

Parsons’s very starting point, his account of utilitarianism, is questionable. The problem here is not that his criticisms of orthodox economics lacked applicability to the work of some economists of his own time, and of other times also, but that he seriously overextended these criticisms. Some had little relevance even in regard to the neoclassicists best known to Parsons, such as Knight, who emphasized wants rooted in “ideal values” and “norms having imperative quality” (1933, pp. xiii–xv), or

¹³ There is an asymmetry in the way *Structure* develops this argument. When treating the transition from positivism to voluntarism, Parsons’s analysis proceeds on the *substantive* front and centers on how the value factor came to penetrate the positivist-utilitarian framework. When considering the transition from idealism to voluntarism, however, Parsons is interested in the “breakdown of the idealistic methodology,” which denied “that the sociocultural sciences”—the sciences of the value factor—“make use of ‘general laws’ of the logical character [found] in the natural sciences” (*S*, pp. 580, 719 [emphasis added]; see also O’Neill 1976, p. 296). For reasons discussed in Sec. III, such a denial was unacceptable to Parsons, and his examination of Weber focuses accordingly on Weber’s criticism of this methodological position, *not* on the process by which a theorist concerned with value factors came to encompass positivist-utilitarian considerations. As Parsons writes: “The justification for dealing with Weber in connection with the ‘idealistic’ tradition lies in the fact that *though his own [starting] position does not fall there*, [he] oppos[es] the commonest methodological doctrines of that school” (*S*, p. 580 [emphasis added]).

Taylor, who held that “it is *only within* a strong framework . . . of socially enforced rules limiting the ‘rational’ pursuit of private action, [that the latter] can go on in the ways . . . economic theory describes” (1936, p. 390). Nor were such statements isolated qualifications; they formed recurrent themes. But Parsons’s analysis emerges as still more deficient for many of the earlier utilitarians. Research on Hobbes, Locke, and Spencer, for example, has absolved each of them of reasoning within the confines of the utilitarian dilemma (Barry 1970, p. 77–78; Giddens 1977, p. 210; Peel 1971, p. 238; Perrin 1976, p. 1351; Scott 1962, p. 54; Warner 1978, p. 1322), and it has been shown, too, that these thinkers were “atypical exemplars” of the larger utilitarian tradition, the writings of which belie charges of randomness on ends, a latent problem of order, and so on (Mayhew 1984, p. 1288; Camic 1979).

These are not minor historical corrections. The weak fit between Parsons’s conception of utilitarianism and the ideas of many of its representatives undermines his portrait of three centuries of European thought as a struggle with the utilitarian dilemma that led thinkers finally to turn to value factors and to sociology. Seriously undermined as well is the often-asserted usefulness of the dilemma as an analytical heuristic. The reason many actual utilitarians avoided the utilitarian dilemma is that, besides incorporating many of the elements Parsons claims they neglected, they took account of factors—notably, motives, communicative social interaction, and political processes—that he himself slighted when framing the dilemma (see Sec. VI and VII). The dilemma is not, in other words, a statement of the options available to utilitarian theory in general but a projection onto the history of European thought of the predicament posed for Parsons by the contemporary intellectual scene. Having embraced the dichotomy of the normative and the biological, Parsons has no conceptual place in *Structure* to situate economic theory except on the uncertain middle ground between the dichotomy’s two poles, with the result that limitations in the theory are interpreted as reflecting its “unstable” location. So positioned, economic thought, to the extent that it does not move in the desired normative direction, can only remain unstable or pull toward the factors of heredity and environment; no other possibility is recognized as other than a temporary way station on the line to the biological domain.

But set aside Parsons’s analysis of utilitarianism, and his historical argument grows no more cogent. Concerned though he is with charting a great transformation in European social theory, Parsons prefers an in-depth analysis of Marshall, Pareto, Durkheim, and Weber to the pretense of “encyclopedic completeness,” reasoning that “omissions are not relevant unless [it is shown that their inclusion] would significantly alter the conclusions” reached (*S*, pp. 15, 20). For many of Parsons’s omissions,

however, this is what has been shown. It has been established, for instance, that the theoretical position that he posits as the epochal achievement of thinkers after 1890 appears to be such only because he treats no earlier period in equivalent detail. Yet an appreciation of the interplay between value factors and “utilitarian” or conditional elements has been found to characterize “nearly all the philosophic and religious thinkers of Western civilization” since Aristotle and to be clearly present—a century before *Structure* dates it—among “the British utilitarians, the French positivists, and the German idealists,”¹⁴ as well as those at the crossroads, like Marx (Levine 1980, pp. xxii, xix, xxiii; see also Alexander 1983b, p. 155; Ford 1974; Giddens 1976a, p. 727; Gould 1981; O’Neill 1976, p. 299). While Parsons’s pre-*Structure* writings actually evince some awareness of this (see 1934a, p. 230; 1935b, p. 427), his book does not. Programmatically committed to showing that recent decades were witnessing a move away from theories concerned, like economics, with a single set of factors, *Structure* proceeds as if the whole pre-1890 era consisted of little else but so many variants of economics.

But even if attention is restricted to the post-1890 period, significant omissions remain in Parsons’s argument, spawning its most serious shortcoming: its denial that the sociological tradition was rooted in a variety of theoretical approaches, which developed diverging perspectives *despite* agreement on the importance of normative and non-normative elements. This denial is accomplished by a corollary of the “convergence thesis” that portrays ostensible differences between Marshall, Pareto, Durkheim, and Weber as giving way to a more basic consensus that also represents “a movement of major proportions extending far beyond the works of the[se] few men” (*S*, pp. 14, 774–75). Parsons does not feel called upon to justify the assumption that those he disclaims treating are, in fact, in accord with those he does examine. He carries the point rhetorically by reporting that his four subjects were selected not to corroborate the convergence thesis, but because they were concerned with “the modern economic order”—that they happened to uphold “a common conceptual scheme . . . only very gradually . . . became evident” while he was studying their writings (*S*, p. xxii). The years before *Structure* reveal, however, the more complex story of Parsons’s careful construction of the convergence argument, formulated in the early 1930s as a “Marshall-Pareto-Weber ‘convergence’” (Parsons 1970, p. 828; Wearne 1981, pp.

¹⁴ *Structure*’s treatment of positivist thought says almost nothing about what is ordinarily called the positivist tradition in social theory. For Parsons, the exemplars of positivism are Godwin, Malthus, the Darwinists, and the behaviorists, not Saint-Simon, Comte, and their followers—though Parsons is, as usual, willing to suggest that the latter were in essential accord with those he does examine (see *S*, pp. 110–21, 307). For refutation of this suggestion, see Ford (1974) and Levine (1980, pp. xiv–xvi).

821–22); altered in lectures a few years later with the addition of Durkheim, Simmel, Tönnies, and Znaniecki and the substitution of Hobhouse for Marshall;¹⁵ stated in 1935 without reference to Hobhouse and Znaniecki (Parsons 1935a, p. 283 n. 1); and finally recast in the book, with Marshall reinstated, Tönnies relegated to a short note, and Simmel eliminated—among other reasons because, as Parsons subsequently explained, the Simmelian “program did not fit [the] convergence thesis” (1979 letter to J. C. Alexander).

Parsons did not mean by this remark that Simmel was unaware of the role of normative and non-normative factors. His point was that, despite this awareness, Simmel’s sociology was not very useful for backing up the historical case his charter was making against economic theory, the ability to do so being the *de facto* criterion for settling on Marshall, Pareto, Weber, and Durkheim (1979 letter to J. C. Alexander; *S*, p. 13), each of whom might be described as moving beyond economics into the sociological domain of norms and values. That important contemporaries of the four fell short by this criterion and were thus, like Simmel, set aside was a tacit admission by Parsons himself of the inadequacy of assuming that a consensus on the part of his quartet represented a general historical movement toward the same consensus. That no such movement ever occurred has elsewhere been documented more directly, with scholars demonstrating that “far from providing a compellingly unified framework, [the theorists of the late 19th and early 20th centuries] produced a gallery of sociologies based on radically different conceptions of principles, methods, aims, and subject-matters for the field” (Levine 1980, pp. ix–x, xx). Especially excluded from Parsons’s account,¹⁶ commentators have shown, are thinkers concerned with social-psychological processes, particularly interpersonal interaction and communication (Freud, Dewey, Mead, Thomas), those concerned with political structures (Tocqueville, Michels, the later utilitarians), and those theorizing social structure in terms of a relational conception of the social, whether in Simmelian transactional form or in the Marxist form of class relations of production (Alexander 1983b, pp. 215–17; Atkinson 1972; Bottomore 1969; Clarke 1982; Faris 1953; Giddens 1976b, p. 19; 1982, p. 77; Gould 1981; Hinkle 1963; Levine 1971, 1980; O’Neill 1976; Turner 1987; Wearne 1982, 1983; Zaret 1980).¹⁷ These exclusions, which express what will emerge below as

¹⁵ Lengthy notes of these lectures, taken in the 1934–35 academic year by Stillman P. Williams, are available in the Harvard University Archives.

¹⁶ Also excluded are phenomenological and historicist thinkers out of step with Parsons for the methodological reasons discussed in Sec. III.

¹⁷ *Structure* did not, to be sure, completely neglect Marx himself (see *S*, pp. 108–11, 488–95). It views Marx’s thinking, however, as “fundamentally a version of utilitarian individualism,” albeit a version sensitive to the “power relationship between the

American Journal of Sociology

Structure's overall view of the social-psychological, the political, and the relational, make clear that "the validity of [the Parsonian] approach . . . cannot rest upon its historical triumph" (Alexander 1983b, p. 215).

The conclusion is reinforced if one turns from the figures Parsons excluded directly to Marshall, Pareto, Durkheim, and Weber. The treatment *Structure* offers of the four has justifiably received wide praise for bringing to the task of textual interpretation far more analytical rigor than had any earlier American sociologist, for affording insights into the works discussed that even specialist commentators have continued to find fruitful,¹⁸ and for opening a dialogue among thinkers previously cordoned off in separate "schools" that has proved—especially with Durkheim and Weber, whose writings were little appreciated before the book—to be an enduring wellspring for the sociological imagination (see, e.g., Giddens 1982, p. 76; Martel 1979, p. 616; Shils 1970, pp. 785, 789).

What has also emerged, however, is that in striving to make his four subjects exponents of the voluntaristic theory of action, Parsons reached conclusions that their works do not sustain. His analyses of Marshall and Pareto have thus been found to misrepresent the structure of their thought by imputing to them concerns (with normative orientations and social order) that are not salient while overlooking themes (value cleavages and conflicts, structured economic and political inequalities) that are central (Bierstedt 1938a, 1981, pp. 406–8; Lopreato 1971, pp. 319–20; 1980, p. xxiv; Mulkay 1971, p. 90; Pinney 1940, p. 117; Whitaker 1977, pp. 464–72). *Structure* has been similarly criticized for forcing Durkheim too much into the mold of its larger argument, wrongly rendering his early work as a critique of utilitarianism that collapsed into biologistic positivism, and then so uniformly portraying his mature writings as concerned with action theory and the value factor that the "institutional component of Durkheim's analysis becomes almost entirely lost to view," taking with it his treatment of economic and political structures and of the genesis of normative elements in collective patterns of interaction and integration (Giddens 1977, p. 211; see also Alexander 1982b, pp. 427–29;

classes" (*S*, pp. 110, 489). To Parsons, Marx "shares [the classical economists'] preoccupation with the means and conditions of action, hence the . . . assumption of the randomness of ultimate ends"; "a common value element" is generally lacking in his work (*S*, pp. 493–94). In Parsons's eyes, this is a fatal flaw and classes Marx as a "politico-economic" theorist, not part of his sociological mainstream (1934b, p. 543 n. 8). For discussion of Parsons's attitude toward Marx, see Buxton (1985, pp. 54–60), Gould (1981), Holton and Turner (1986, pp. 37–40), and Wearne (1981).

¹⁸ That this is so among Durkheim and Weber scholars is too evident to require documentation. But the point also applies to students of Marshall and Pareto (in the former case, see Blaug 1962, p. 401; Collini, Winch, and Burrow 1983, pp. 320–21; Pursell 1958, p. 295; Wood 1982, p. xvii; in the latter, Finer 1966, pp. 43, 49).

Bierstedt 1981, pp. 408–9; Cohen 1975; Giddens 1972, pp. 39–43; 1976a, pp. 706–9; Pope 1973, 1976, pp. 34–42; Rex 1961, pp. 99–101; Traugott 1978, pp. 21–36).

Structure's interpretation of Weber has fared no better. Here the recurrent objection has been that Parsons's "overweening emphasis on the category of the normative . . . led him to expand what is but a part of Weber's sociology [into] very nearly the whole," particularly by reducing his qualitatively different types of social action, legitimate order, and so on all to variations of the normative factors, converting his analysis of cultural beliefs and their intrasocietal diversity into an account of social integration through shared values, and concentrating on his religious writings to the neglect of his economic and political sociology (Cohen et al. 1975a, p. 240; see also Alexander 1983a, pp. 183–85; Camic 1986, pp. 1074–75; Cohen et al. 1975b; Collins 1968, pp. 47–51; Horowitz 1964; Zaret 1980). Parsons has been faulted likewise not only for imputing to Weber, and to Durkheim, an overly normative solution to the problem of order but also for portraying the two as occupied with the general issue of how order is possible among interest-seeking actors (see Abercrombie, Hill, and Turner 1980, pp. 45–46; Burger 1977, p. 323; Collins 1968, p. 57; Giddens 1972, pp. 41–42; 1976a, p. 709; Levine 1980, pp. x–xi; O'Neill 1976, p. 299).

When the effects of these emendations are summed up, the conclusion that emerges is that, far from demonstrating deep theoretical consensus, the works of Marshall, Pareto, Durkheim, and Weber document the great diversity that constituted the history of sociological thought. Research on Durkheim and Weber, in particular, has emphasized their "almost complete divergence [with respect to] method and substance" (Bendix 1971, p. 297; see also Collins 1968, 1985; Pope, Cohen, and Hazelrigg 1975, 1977), while related scholarship has indicated that, even where the two theorists shared complementary concerns, they pursued these in ways that exhibit little theoretical convergence (see esp. Giddens 1971, 1976a, 1987). The task of sorting out the differences and similarities between Weber and Durkheim (and their contemporaries and predecessors) has, in any event, been shown to entail an analysis that takes account of the specific issues engaged by these thinkers as each came to terms with his particular sociohistorical, intellectual, and institutional context (see Burger 1976; Jones 1977; Lukes 1973; Peel 1971, p. 240).

Structure deliberately eschews this kind of analysis—and does so for reasons that lead finally to the question of why the book is so insistent that all sociological traditions converge on a common conceptual scheme. At the time that he wrote, Parsons saw himself engulfed by a "strong current" of belief "that there are as many systems of sociological theory as there are sociologists, that there is no common basis, that all is arbitrary

and subjective.” To him, this belief was an assault on his whole enterprise, for it implied that “sociology is an art,” not a discipline to be chartered as a science (S, p. 774). The belief needed, therefore, to be extirpated, and toward this end *Structure* poses as a study of “the process by which scientific thought develops”: a study seeking to establish that social theories develop the same way that (according to Whitehead, Henderson, and the neoclassicists) genuine science advances, that is, from the interaction of theory and facts, the product of which is a progressive approach to the “finite totality” of possible knowledge about a given aspect of reality (S, pp. 11, 601, 697, 725–26). For Parsons, the convergence thesis was decisive proof of this, a way of allowing that his subjects were embedded in different contexts and had different “centers of attention” and “emphases,” but then annulling these allowances with the assurance that, “if we but take the trouble to dig deep enough,” what appear in place of “arbitrary subjective judgments” are shared theoretical concerns and a common theoretical framework (S, pp. 5, 27, 490, 717, 720, 774–75). Even utilitarianism, positivism, and idealism, one-sided though they were, had marched in step; each left a “permanently valid precipitate” that the voluntaristic theory of action integrated to form a more encompassing conceptual scheme (S, pp. 125, 775, 781). This process of development, Parsons maintains, is analogous to the way theoretical physics itself advanced; “it is scientific progress” (S, pp. 470, 775).

This was a powerful brief on behalf of sociology, the perfect Whig history: generations of thinkers engaged in an “immanent” theoretical development that overrode the effects of time and place and culminated in the very position Parsons required in his campaign to make way for a science of the value factor. The empirical limitations of this contention have been outlined above. But even more telling is the simple methodological point—which bears not only on Parsons but also on those who counter his interpretation with other convergence arguments—that with flexible enough interpretive standards, it is relatively easy, as Mulkay has observed, to prove assorted convergences “among virtually any group of theorists” (1971, p. 69; see also Jones 1977, pp. 284–87). That turn-of-the-century European thought revealed to Parsons the trend that it did had, as should now be clear, chiefly to do with his own situation in the American debates of the interwar years. His struggle, to go beyond economics in the normative rather than biological direction, so occupied him that “he retroactively interpreted the [whole] sociological tradition in terms of that struggle” (Levine 1980, p. xxvi; see also Abercrombie et al. 1980, p. 54; Gouldner 1970, p. 17; Hawthorn 1976, pp. 214–15; Vidich and Lyman 1985, p. 301)—generalizing the particular once again. More than three decades after *Structure*, Parsons said as much. Pressed by critics, he conceded that other convergence theses might readily have been estab-

lished; that the conceptual scheme at the heart of his own convergence thesis was “in considerable part [his] contribution, not [an] induction” from the works of his subjects; and that he did neglect a broad array of theorists, overbiologize the early Durkheim, slight Weber’s political sociology, and more. All of this could be explained, however: “I had special reasons in the design” of *Structure*, wrote Parsons, “to emphasize the importance of the normative,” reasons involving the “clarif[ication of] the relation between economic theory and sociological theory,” and this “particular problem complex” must be borne in mind to “specify the limits” of the book’s historical argument (1968, pp. viii, xiv n. 10; 1975a, pp. 109; 1975b, pp. 666–68; 1976b, pp. 363–64; 1978, p. 1351). In the nature of the case, that argument appears in the charter itself without this specification.

V

Anchoring Parsons’s intellectual-historical discussion is his concept of action. *Structure*’s “empirical subject matter [is] human action in society,” writes Parsons, because “it is a fact that men assign subjective motives to their actions, . . . express philosophical . . . ‘ideas,’ [and] associate these ideas [with their] motives. It is a fact [also that men] manifest [their] subjective feelings, ideas, motives [through] symbols to which meaning is attached. This subjective aspect involves the reasons we . . . assign for acting as we do” (S, p. 26). This point of view was in explicit opposition to the “objectivist trend [within] behaviorism.” Everything that the behaviorist movement denied, Parsons’s charter affirmed with a swoop of its dichotomous logic that now shunted “behavior” and cognate terms aside, while agglomerating all the counterforces—action, subjectivity, ideal motives, symbols, meanings, reasons, and (as other passages add) purposes, means-end relationships, and free rational and moral choices—with little effort to disentangle or define the multivalent expressions (see 1935a, pp. 282–83, 289, 294; S, pp. 44–46, 77–88). In this, Parsons was conforming, again, to the practices of the debates of the time and taking sides against a very real enemy; he did not have call for the distinctions a European thinker like Schutz (1932) was then making (in work familiar to him) between subjective meanings, reasons, causal motives, and so on.

Early in his analysis, however, Parsons does bring out one of two far-reaching decisions underlying his conceptualization of action. From all the terms that he counterposed to objectivism, he focuses on one concept in particular: “the means-end relationship seems,” he declares, “to be fundamental to all consideration of action from the subjective point of view” (S, pp. 422–23). *Structure* does not question this assimilation of the

American Journal of Sociology

subjective to the means-end relationship; it is thus vulnerable to the objection, of Schutz and others, that it “never asks what really does happen [from a] subjective point of view, [since it uses not] truly subjective categories, but . . . only objective categories for the interpretation of [the] subjective” (Schutz 1940–41, p. 36; see also Hall 1984; Heritage 1984; Wagner 1979; Zaret 1980). Before *Structure*, Parsons had raised a similar issue and warned against the “arbitrary assumption that [the means-end] schema can exhaust the subjective aspect” (1935a, p. 284). But this reservation falls to the side amid *Structure*’s campaign to prove that sociology, like the established sciences, possesses an encompassing analytical framework. In the book, Parsons insists that, just as “it is impossible to talk about physical processes [without] the space-time framework of physics,” so “it is impossible even to talk about action in terms that do not involve a means-end relationship” (S, p. 733).

In placing the emphasis here, Parsons joined forces with critics of Watsonian behaviorism like Edward Tolman, who, in research that Parsons recalled “particularly impressed [him] at the time” (1974, p. 56), had identified “means-end strands” everywhere, even defining “right and left, [and] good and bad [as] but *means-end affairs*” (Tolman 1932, p. 779). The idea obviously resonated, too, with the approach to action that Parsons had encountered in neoclassical economics and that had been well systematized by this point, for instance, in work he knew by the Harvard economist Allyn Young, who proposed treating human conduct in a “scheme of abstraction” consisting of four elements, besides certain “givens” from the “physical environment”: (1) agents; (2) ends; (3) instruments, or means for attaining ends; and (4) “a mechanism,” or “a set of social processes,” connecting means to ends in accord with “communal interests,” which may “impose conditions upon [an agent], prevent him from doing certain things, [or] encourage him to do others” (Young 1927, pp. 3–4).

Structure takes off along similar lines with its widely cited discussion of the “unit act.” Parsons states: “An ‘act’ involves logically the following: [1] An agent, an ‘actor.’ [2] An ‘end,’ a future state of affairs to which the process of action is oriented. [3] A ‘situation’ [differing] in one or more respects from the state of affairs to which the action is oriented, the end. The situation is analyzable into two elements: [i] those over which the actor has no control, [that is,] the ‘conditions’ of action; [ii] those over which he has such control, [that is,] the ‘means.’ [4] Finally . . . there is . . . a certain mode of relationship between these elements. That is, in the choice of alternative means to the end . . . there is a ‘normative orientation’ ” (S, p. 44). In considering this definition, one must beware of anachronistic projections, especially regarding the term “conditions.” Alexander, for instance, equates the concept with material social conditions

in a sense similar to material conditions in Marxism (see 1978, p. 1979; 1983b, p. 12–37; 1987b, p. 24). But *Structure* expressly rejects this view, dissociating Marxist materialism from “the prevailing Western sense of the same term,” which refers to “the nonhuman environment, as natural resources, or [to] biological heredity” (*S*, pp. 490–91). According to Parsons, the conditions of action are not “the concrete conditions of a particular concrete act” or the actor’s actual “social environment,” but the “ultimate analytical conditions of action in general,” that is, the nonsubjective categories of “heredity and environment in the biological sense”—the sense uppermost in the controversies of the period (*S*, pp. 71, 82, 252, 267, 364, 700; see also Gerstein 1979, p. 28; Procter 1978, 1980).

When this is realized, it becomes clear how Parsons’s conceptualization of the unit act weaves into the framework of theorists like Young the normative/biological = conditional axis of his own study. The desired emphasis on means and ends is thus maintained and brought into line with *Structure*’s “great dichotomy.” This accomplished, Parsons proceeds to a more general characterization: considered analytically, “action must always,” he writes, “be thought of as involving a state of tension between . . . the normative and conditional [elements]. As a process, action is, in fact, [an] alteration of the conditional elements in direction of conformity with norms” (*S*, p. 732), or, less technically, the “eternal struggle to achieve ideals in the face of a stubborn and resistant natural world” (to quote Procter’s useful formulation [1978, p. 47]). It is not apparent how this conception would accommodate the idea—uncommon, to be sure, in particular cultures familiar to Parsons, but known elsewhere—that “tension” and “struggle” are *not* the basic forms with which to posit the relation between natural conditions and human agency. What is evident, however, is the close fit between the polemical project of *Structure* and its underlying vision of action. Setting out to vindicate a science of the normative factor from the onslaught of behaviorist theories that gave primacy to conditional elements, Parsons conceives human action itself as ultimately a normatively charged effort to triumph over the obduracy of the conditional.

This conception brings to the fore Parsons’s second decision about action: his belief that all action has a normative dimension: “Just as there is no such thing as motion except as change of location in space, . . . there is no such thing as action except as effort to conform with norms” (*S*, pp. 76–77). Historically speaking, the case *Structure* builds on behalf of this proposition is the achievement of the book. For reasons suggested below, it is an achievement we now do well to view with caution, though this does not diminish the accomplishment. Like other major social thinkers, Parsons (as Berger once remarked [1962, p. 512]) manages to “corrupt the innocence with which we view [what his] theory touches”: after reading

American Journal of Sociology

Structure, one has difficulty looking upon human conduct as one did before, for normative elements suddenly seem to be at work everywhere. In this respect, Parsons outdid many other American sociologists of the 1920s and 1930s, bringing an array of observations—each in itself fairly commonplace in critiques of behaviorism and neoclassicism—within the orbit of a single theoretical scheme that highlighted the normative component in everything from rational daily activities to nonrational religious rituals (see esp. *S*, pp. 210–11, 430–31).

This feat of integration is facilitated by *Structure's* elastic, and programmatically useful, employment of “normative” to mean (i) any “determinate selective factor” applied by an actor when choosing among alternative means; (ii) “a sentiment attributable to one or more actors that something is an end in itself”;¹⁹ (iii) “a verbal description of [a] course of action . . . regarded as desirable, combined with an injunction”—or “obligatory” rule—to make [action] conform to this course”; and (iv) any “embodiment” of the value factor, that is, of society’s ultimate common values and more diffuse value attitudes (*S*, pp. 44–45, 75, 259–60, 387, 404, 446, 464; see also Schutz 1940–41, pp. 30–32; Sorokin 1966, p. 412). There are passages in *Structure* that recognize that these meanings may not coincide; the book describes the economists’ “rational norm of efficiency,” for example, as consistent with usage i yet remote from usage iv (*S*, pp. 56–59, 77–80). But this is unusual. More typically, Parsons confounds the different senses of the term and, on top of this, reduces them all to the fourth of the listed meanings, thereby downplaying the slippage between a factor involved in the choice of means and sentiments about what is an end in itself, and between such sentiments and the stated rules regulating courses of conduct, and between these rules and shared ultimate values.

Given these reductions, it is not surprising that *Structure's* argument for the normative dimension of action has frequently been interpreted as the claim that all concrete action is governed by common values and attitudes. Parsons’s analysis has then been faulted for neglecting that “people may share [values], while remaining profoundly different in . . . their overall pattern of behavior” (Swidler 1986, p. 275); that actors “construct [their] action, instead of merely releasing it” (Blumer 1969, p. 64); that they are less occupied with “following” prescribed rules than with “develop[ing] what a rule means” (Zimmerman and Wieder 1970, p. 292); that there is a “negotiated character [to] norms, [which] are open

¹⁹ This second definition, the most formal provided in *Structure*, reproduces a definition offered to Parsons in an October 1936 letter (now in the Harvard Archives) from L. J. Henderson, who in reading a draft of the book had been troubled by its imprecise use of “normative.” Interestingly, although Parsons adopted Henderson’s formulation, he seems not to have reworked his text in light of it.

to divergent and conflicting ‘interpretations’ in relation to divergent and conflicting *interests*” (Giddens 1976b, p. 21); and that a great deal of human conduct is impelled by full-blooded self-interest itself (Homans 1964, p. 814).

Not all of this was lost on Parsons. He recognizes that an “element of embellishment” often accompanies normative compliance, that such compliance frequently derives from a “morally neutral” concern with avoiding punishment, and that “the ends of the great majority of practical activities are . . . far removed from ultimate values” and closely tied to mundane “interests” (S, pp. 401–4, 465, 679). But the only one of these points that he pursues is the significance of “interests”—and here one must bear in mind, against the trend of recent scholarship (see, e.g., Münch 1981, 1987), that Parsons uses this term in a highly unusual way. The burden of his whole discussion of the utilitarian dilemma is that concepts like interests are not freestanding but unstable; when rigorously analyzed, they give way to two, more elemental sets of factors—heredity and environment on the one hand, normative value elements on the other. Building on claims developed in an unpublished paper from the mid-1930s (see 1935c, pp. 12–14), Parsons in fact maintains that, to the extent that “interests” are not “ideal interests” based on common value attitudes, they are to be understood as rooted in “desires,” “appetites,” and “passions,” which originate in instinct and other aspects of individual “human nature” as studied within the biological sciences (S, pp. 47, 402, 436, 465, 572, 685).

Full-blooded interests, in the bluntest sense, are not, then, a problem for the book’s analysis of action. Its real difficulty is its inability to make room theoretically for socially constituted interests that are not resolvable into common normative factors, or, indeed, for any aspect of the means-end relationship that is not to be fitted into the polemical categories of heredity-environment versus common norms. Convinced by the polarized situation in which he perceives himself that the “normative aspect characterizes the concepts of all [viewpoints] on human conduct in so far as they transcend behaviorism” (1935c, p. 3), Parsons simply has nothing beyond normative factors to appeal to once he calls into question the sovereignty of conditional factors. It is through the same dichotomous reasoning, moreover, that he is led to lump together the different meanings of the term “normative” and to merge them all with the factor that guarantees the independence of the normative from the conditional: common cultural values. In this sense, there is justice in the critics’ charges—though irony as well, for Parsons’s move to theorize action only in terms of common normative values (and conditional elements) has been met with counterclaims that take the same blanket form in dismissing the normative or conceptualizing it as something else altogether. The global argu-

American Journal of Sociology

ment of *Structure* has tended, in short, to encourage those concerned with means-end affairs to wax global themselves, not to examine the *variable* role of normative elements, *and* normative elements of various sorts, under different social circumstances.

These observations pertain to Parsons's decision to view means-end relationships as having a normative dimension, but related issues arise regarding his prior decision to cast all action in terms of the means-end relationship. It is no exaggeration to say that *Structure* has been among the principal sources of the tendency of sociologists in the past three decades—even sociologists most critical of Parsons's normative emphasis—to take this decision for granted, to see it not as a decision at all but as the natural baseline for the analysis of action. But it remains true that, in identifying the means-end schema as the space-time framework of the social sciences, Parsons was giving foundational significance to the specific perspective that he had acquired from the neoclassicists and that had become a strategic weapon in the war on behaviorism.²⁰ As Taylor points out, Parsons “abstracted the [means-end notion from] neo-classical economics [and] then elevated it to the status of a concept with universal application, . . . discarding the limits placed on its application by neo-classical economic theory” (1979, p. 9; cf. Coser 1977, p. 562; Lidz 1986, p. 157). This had two far-reaching consequences for the approach to action laid down in *Structure*.

The first of these is its homogenization of those actions that may be fitted into the means-end framework. As Garfinkel (1987) has observed, in seeking to subsume a myriad of human activities, from speaking, driving, and learning to proving theorems and taking medicines, all as instances of the means-end relationship, Parsons divests each activity of many of its characteristic sociological properties, leaving each as faceless as the utilitarians allegedly left ends (see also Whalen 1987). It is true that there are grounds on which to defend this divestment; by claiming that actions uniformly exhibit a “common structure” and vary only in the content of their norms, means, and ends (*S*, p. 734), Parsons directs attention to similarities useful to appreciate in some theoretical contexts. For the purpose of understanding particular social situations, or historical variations therein, however, it is necessary to recognize that there are limits to this kind of leveling approach. Until we do, we will continue to lose sight of the “organic character” of human activities: of the fact that actions are not simply homologous means-end pairings but also diverse

²⁰ Cf. Levine's observation: “Parsons . . . restricts the meaning of action to that of a single tradition, one that builds on the means-end schema of utility-seeking individuals, ignoring important alternative ways of construing action such as those of Herder and Dilthey or Dewey and Mead” (1986b, p. 1238).

“larger assemblages,” or complex modes of involvement in bounded arenas of social and material objects—that is, historically changing *practices*, economic, political, scientific, legal, artistic, moral, familial, and so on (Hall 1984, p. 268; Swidler 1986, p. 276; see also Giddens 1982, pp. 108–15).

A second consequence of Parsons’s universalizing of the means-end schema is his virtual occlusion of those actions that cannot be fitted into the schema. The point here is not simply that the schema suppresses the specificity of particular actions, but that “there are many for [which a means-end] description is quite inapplicable” (Taylor 1979, p. 10; see also Black 1961, p. 281; Menzies 1976, pp. 31–46; Sorokin 1947, pp. 44–47; 1966, pp. 413–14; Znaniecki 1952, pp. 193–96). This has been shown especially for actions that are predominately emotional and habitual, cases *Structure* glosses over by merging emotion with the value factor and habit with the biological factor of the behaviorists (*S*, pp. 116, 646–47, 671–72; see Becker 1950, pp. 13–14; Berger and Luckmann 1966, pp. 53–67; Camic 1986; Kemper 1981). In addition, neither actions that assume a means-end form, nor those that do not, occur under all circumstances. Criticizing Parsons for projecting an “action schema [that] is a culture-bound approximation to behavior in Western societies” onto more traditional cultures, Gouldner (1955, p. 177) once demanded a specification of the schema’s “reference period”—and in light of Braudel’s (1979, pp. 28, 561) estimate that 80%–90% of the world’s population has generally acted otherwise, such a demand is no quibble. Even when the reference period is our own, Smith has argued, the means-end model of *Structure* is of uneven relevance, working well for men in certain organizational settings but breaking down for those outside these settings, such as the housewife who is locked in to an action mode of “episodic discontinuity[y],” as she “coordinat[es] the threads and shreds of several lines of action, the projects of more than one individual, while herself pursuing none” (1979, pp. 150–53). This possibility, too, was something that Parsons’s pre-*Structure* writings partially acknowledged in speaking of actions “very difficult, if not impossible, to fit into the . . . analytical straitjacket [of the] means-end schema” (1935a, pp. 309–11). But there are no comparable qualifications in *Structure*. In accord with its goal of chartering sociology by the scientific standards of the time, the book becomes a manifesto for a generalized *theory* of action—which was duly opposed, in later decades, by competing *theories* of action—not a *sociology* of action that would take action modes as variable and seek to understand the sociohistorical circumstances in which means-end forms (seen as diverse ensembles or practices whose normative component is variable) and various non-means-end forms (normative and otherwise) prevail, and why.

VI

Because it takes the unit act as a point of departure, *Structure* has often been misunderstood. In the past few years alone, for example, Coleman (1986, pp. 1309–11) has portrayed the study as a tract for “methodological individualism,” while Turner (1987) has faulted it for its neglect of the “social system” and “social structure” and Habermas (1981a, pp. 179–81) for its lack of “a mechanism to explain the emergence of a system of action out of unit acts.” This line of interpretation originated several decades ago,²¹ though it has yet to confront the counterevidence identified by other scholars,²² whose research, when linked to a contextualized understanding of *Structure*, prepares one for what Parsons saw as his book’s central theoretical argument.

Aside from a few passing illustrations, *Structure* exhibits little interest in applying the unit-act scheme to the “concrete, actual act[s]” of individual actors (*S*, pp. 44, 48) or in using it to address the questions theorists with this interest have sometimes raised about the unintended consequences of purposeful actions and the like. The scheme had been used in these ways, Parsons believed, by the utilitarians and positivists, but this had fostered “atomism,” the idea that “the properties of systems of action” can be inferred by “direct generalization from . . . isolated unit acts,” and “individualism,” the idea that system properties are “predicated on [facts concerning] isolated ‘individuals’ combined with a process of direct generalization from these facts” (*S*, pp. 52, 72). Both tendencies had long impeded the emergence of sociology, and it therefore was Parsons’s intention to counteract them, *not* to abet them. To this end, he advocated an “analytical,” rather than “concrete,” use of the unit-act framework (*S*, pp. 48–51). In a pivotal section of *Structure*, widely overlooked because of its location in his chapters on Pareto, Parsons insists that, viewed analytically, a unit act is only an “abstraction” from a “total system of action” (*S*, pp. 204, 230): “Actions do not take place separately . . . but in long, complicated ‘chains’ so arranged that what is from one point of view an end . . . is from another a means to some further end and vice versa . . . through a great many links in both directions. . . . Any concrete act may constitute a point of intersection of a number of such chains. . . . Or, to change the figure, the total complex of means-end relationships is . . . to be thought of . . . as a complicated web (if not

²¹ See also Martindale (1959, pp. 40–41), Menzies (1976), Mullins (1973, p. 53), and Tirayakian (1980, p. 18).

²² Namely, Adriaansens (1980, pp. 60–61), Alexander (1983b, pp. 19–21; 1987b, p. 34), Alexander and Giesen (1987, pp. 21–25), Atkinson (1972, p. 12), Devereux (1961, pp. 14–20), Hamilton (1983, pp. 78–80), Holmwood (1983, pp. 315–18), Luhmann (1982, p. 55), Münch (1981, pp. 718, 730; 1982, p. 780), Procter (1978, p. 47; 1980), Savage (1981, pp. 101–5).

tangle). . . . The isolation of any particular chain involves abstracting from these criss-crossings of many different chains" (*S*, pp. 229–30).

Continuing in this vein, Parsons averred: "In following through the chain . . . in one direction—from means to an end, which is . . . a means to a further end . . . necessity leads sooner or later to an *ultimate end* [that] cannot be regarded as the means to any further end. . . . Followed in the reverse direction, . . . sooner or later elements are encountered which must be regarded as *ultimate means or conditions*" (*S*, pp. 230–31 [emphasis added]). The area between these two extremes is "*the intermediate sector* of the means-end chain," which is in turn divisible into "three well-defined" subsectors or categories, the technological, the economic, and the political (1934b, p. 525; *S*, p. 239). These terms are defined succinctly in one of Parsons's early articles: "The technological element exists, in so far as action is concerned, with the choice and application of means for a single end in abstraction from others. The economic element enters when the question of the alternative uses of scarce means for different ends arises. . . . The political element . . . concern[s] the relation of individuals to each other as potential means to each other's ends [and] is present in so far as [control] over others . . . is secured by means of coercive power" (1935a, pp. 293–94; see *S*, pp. 238–41, 265–68).

For Parsons, this schema of ultimate conditions, an intermediate technological, economic, and political sector, and ultimate ends constitutes the central core of the theory of action in *Structure*, as well as the rationale for the book's misunderstood title. According to Parsons, the categories "ultimate ends," "economic subsector," "ultimate conditions," and so on refer not to "concrete phenomena," but to the "analytically separable aspects" or elements of "any system of action"; no element is "even conceivable as independently existing." Taken together, therefore, they may be seen as defining the "anatomy" or, less figuratively, "the *structural aspect* of systems of action. . . . Hence the title 'The Structure of Social Action'" (*S*, pp. 39, 173–74, 230 n. 1, 619 [emphasis added]; also 1940–41, p. 73).

Contrasting this interest in "the structure of social systems of action" with the concerns of "atomistic theories," Parsons expressly embraces an "organic" model of society and then undertakes to identify the "emergent properties of [action] systems" (*S*, pp. 238, 353–54, 734, 739–40). Using the terminology just introduced, he argues that only when attention is confined to the "technological" subsector and the problem of efficiently "choosing the means 'best adapted' to a single given end" is it reasonable to discount emergent phenomena; various "technologies" for mastering nature are sufficient at this instrumental level (*S*, pp. 233, 770). As soon, however, as one turns to the "economic" subsector and the problem of

American Journal of Sociology

apportioning finite means to “a plurality of alternative ends,” it becomes necessary to take account of the emergent property of “economic rationality,” that is, of “rational process[es] for] the acquisition and allocation of scarce resources [among] alternative uses”; the science of economics is needed to investigate this property (*S*, pp. 239, 266, 743, 767). When attention is broadened to the “political” subsector composed of “a plurality of individuals,” another emergent property, “coercive rationality,” which proves to be the domain of the science of politics, makes its appearance as actors seek “coercive power” over each other to achieve their individual ends and “mechanism[s] arise to mitigate this problem through] a relatively stable settlement of . . . power relationships” (*S*, pp. 236–37, 767–68). “But,” Parsons continues, “this coercive rationality . . . opens up a new set of problems, the problems of social order” latent whenever ends are left as individual and given. Such problems can be treated adequately only when one looks outside the intermediate sector of the means-end chain, not however to the conditional elements of the biophysical sciences, but to the “ultimate ends” element lying, at the opposite theoretical pole, in the domain of common cultural values (see Sec. VIII). In Parsons’s view, “common-value integration” must thus be regarded as a final emergent property—and the clinching justification for the science of sociology (*S*, pp. 238–39, 767–69). The argument may be summarized as follows:²³

Structural Aspect of an Action System	Corresponding Emergent Property	Corresponding Academic Field
Ultimate conditions	(None identified)	Biophysical sciences
Intermediate means-end sector:		
Technical element	(None)	Technologies
Economic element	Economic rationality	Economics
Political element	Coercive rationality	Politics
Ultimate ends	Common-value inte- gration	Sociology

This explicitly *structural* schema shows clearly *Structure*’s distance from the asystemic, “monadic” (Habermas 1981a, p. 179) individualism often ascribed to the book. The schema’s underlying idea of crisscrossing means-end chains actually represents a thinking through of the relation

²³ This diagram draws on Levine (1980, p. 1; 1985, p. 122) and Loubser (1976, p. 6).

between “action” and “system” more supple and original than that proposed by certain recent students of the topic (see the collection by Alexander et al. 1987). In addition, the conspicuous place the schema assigns to economic and political elements acquits Parsons of the familiar charge of overlooking them. Value elements receive fuller treatment in *Structure* only because, Parsons (1977, p. 336) later explained, the sociologist selects the specifically sociological factor, leaving the other elements for other disciplines.

There is another side to the story, however. In recognizing that *Structure* affords a more encompassing vision of the social world than it has been credited with, one must also recognize that Parsons’s whole understanding of “structural elements,” “organic” or “emergent properties,” “social systems,” and the like, is bound to his practical situation and charter-building efforts. This is evident in two ways, both of which may be seen as pointing to more defensible reasons than critics have generally offered for demurring at *Structure*’s approach to social phenomena. The first of these is that the structural economic and political elements in Parsons’s schema have highly circumscribed referents. In no sense can these elements be seen as making theoretical way for factors such as modes of production, class structures, state apparatuses, and military force. Structures of *these* types, it has been shown, simply cannot be yielded up by focusing on the emergent properties associated with the functional problems of allocating scarce resources and moderating coercive rationality (see Gould 1981, pp. 211–14; 1987, p. 14; Johnson et al. 1984, pp. 52–53). Significantly, though, Parsons’s early functionalist arguments coexist with a genuine awareness of the brute nature of economic and political life. In work written concurrently with *Structure*, he dwelt at length on the place of “force, fraud, and inequality” in modern society, as exhibited in monopolies, the subversion of democratic state policies by “special interests,” the advantages of “capital and connection” in the upper class, the “inherent bargaining disadvantage” of workers in labor contracts, and “the vicious circle . . . of poverty, low wages, large numbers, and low standard of living” (1936a, pp. 365–69; also S, pp. 104, 109, 288–93, 658).

Yet these are *not* the phenomena that are the focus of attention when *Structure* conceptualizes economic and political factors on the theoretical plane. In the case of the economic factor, Parsons’s desire to make way for a science beyond economics inclines him to preserve, even as he talks of structure and emergence, highly traditional boundaries around the “economic.” For him, as for the neoclassicists, to speak of the economic element is to speak (with analytical abstraction) of conduct in the marketplace of actors who rationally utilize scarce resources to increase their “wealth,” or quantity of satisfactions (see 1936a, pp. 372–79; S, pp. 131,

239, 262; see also Camic 1987). With this conception in place, “force, fraud, and inequality” necessarily become “noneconomic factors,” which impinge upon the economic arena, but are actually manifestations of the political element of coercive power (1936a, p. 372; S, pp. 109–10, 489–90, 657–58). But *Structure* neither examines power in its own terms nor justifies subsuming within it such disparate items as inequality and force, not to mention monopolies, poverty, and so forth. Perhaps because the discipline of political science was still too weakly established to reckon with seriously,²⁴ Parsons treated the political factor far less fully than the economic, presenting power—so as to articulate with his concept of wealth getting—as merely an additional kind of “means” or “interest” (S, pp. 101–2, 262, 298). So understood, it is an element that falls within the broader category of interests (see Sec. V) and, as such, ultimately dissolves into either normative or conditional factors (S, pp. 263, 291, 401). It is, then, in terms of these two sets of factors only, and the normative elements especially, that Parsons treats the state, “the focus of the political element” (1934a, p. 231; 1935a, pp. 293–95). And this example can stand for many others, for he was very willing to carry his polemical dichotomy straight into the social structure. Properties of “social structure, . . . in so far as they are not due to differences of heredity and environment,” he declared when drafting the argument of *Structure*, “derive [from] ultimate values” (1935c, p. 19; also 1934a, p. 231, S, p. 464). It is this belief that turns a schema that is formally multidimensional in its inclusion of economic and political “structures,” in addition to value factors, into a theory centered more exclusively on the latter elements (cf. Alexander 1983b, pp. 213–18; 1987b, pp. 31–32)—and, by so doing, allows Parsons to hold that sociology does more than focus on one among several emergent properties. As it happens, the field’s province is the element that occupies first position in the “hierarchical relation” that exists between common values, power, and economic rationality (S, pp. 240, 249).

Beyond this restrictive view of the economic and political, there is a second way in which Parsons’s conception of the social world is circumscribed by his charter-making efforts. The foregoing discussion will have made clear that Parsons’s concept of an “action system” is neatly

²⁴ At the time, political science occupied a fairly unthreatening academic position, still struggling for autonomy from departments of history in many colleges and universities, though well enough along at Harvard itself for Parsons to address it at least in general terms (see Hart 1930; Somit and Tanenhaus 1967; Waldo 1975). In making power the province of the discipline of politics, Parsons followed contemporary practice. Carl Friedrich, his colleague from the Harvard government department, had, e.g., observed that it is the opinion of “a large group of political scientists in this country [that] the problem of power [is] the focal point of political science” (1937, p. 504).

tailored to the interdisciplinary situation of his time. It proposes that the web of means-end chains is made up of exactly those subsectors, or elements, that stand in direct correspondence to the disciplines that defined the American academic scene to aspiring sociologists of the interwar period. From a programmatic standpoint, such a conception was a useful one. The point to appreciate here, however, is that Parsons takes it as more than this. So long as he adheres to his analytical orientation, his position is that the action system, so conceived, is the social world in its entirety. In Parsons's view, in other words, the schema that suits the hour—that rescues action and welds it to an organic model that both mandates sociology and grants modest concessions to the competition—is what exhaustively defines the domain of social-scientific theory in general, "not only for now but for all time," as Bierstedt (1981, p. 398) has written (see also Levine 1980, p. ix). Parsons's absolute insistence on this comes out particularly well in his neglected but telling critique of Znaniecki's *Method of Sociology* (1934). What attracted Parsons's attention was Znaniecki's argument that sociology occupies a "domain [separate] from those of other sciences" and that this domain is the "social system," consisting of "social actions," "social persons" or "social roles," "social relations," and "social groups" in the sense of organizations and institutions (1934, pp. 105–36 [quoting pp. 130–31]). Znaniecki made the case that while the sociologist could profitably study *any* of these roughly equal "subdivisions," it was not (yet) justifiable to encompass all these phenomena in "a general theory of actions" (1934, pp. 135–36).

This viewpoint was one that Parsons could not accept, primarily because it conceded too much to those who ridiculed sociology for offering almost as many perspectives as there were sociologists. *Structure* seeks, therefore, to defend the proposition that, of Znaniecki's "four schemata, . . . that of 'action' [is not only] the most elementary," but alone has the theoretical capacity to subsume *all* the others, which must "be regarded as secondary to the action schema" (S, pp. 39, 744–47). According to Parsons, concepts like "social relations" and "social groups" are valuable to the sociologist as "shorthands": devices for "describing" a concrete action system without the "laborious work" of breaking it down into means-end chains and their emergent, structural properties (S, pp. 743–44). Such concepts play no real analytical role, however. Indeed, so lightly does Parsons take the "social" part of the "social action" phrase in his title that not until a footnote 768 pages into his book does he explain that "social action" refers to "a plurality of actors mutually oriented to each other's action." But neither this idea nor any of his other nods to the relational alter his belief that the "social relationship schema" contributes nothing at the "analytical" level and is only "another way of looking at the same facts involved in the schema of action" (S, pp. 649, 693–94). The

American Journal of Sociology

same holds for a group or institutional schema: "There are no group properties that are not reducible to properties of systems of action and there is no analytical theory of groups which is not translatable into terms of the theory of action" (*S*, p. 747). The theory's categories actually are so powerful, in Parsons's estimate, that they can grasp, too, "all change and process" in the "action field" and apply to societies primitive and advanced, static and dynamic (*S*, pp. 367, 411, 733). This line of thinking has an important implication.

In the course of intellectual history, the concept of the social has been used to mean different things, among them relations, groups, institutions, patterns of change, and more. By absorbing all these things into schemata held to be reducible to the properties of the theory of action and by arguing, further, that, when theoretically dissected, such properties break down into either normative or conditional factors, *Structure* supplants these meanings with a conception of the social (and the social world) better suited to its practical intentions. For Parsons, the social is, when considered analytically, ultimately nothing other than the normative or common value factor (*S*, pp. 420, 424, 427, 446, 448, 460), which is the linchpin of his charter for sociology. So completely, in fact, does Parsons take the controversies of his age as the measure of social theory in general that, in conceptualizing the social, the central issue is to establish how "the whole 'social' factor swings over from the category of . . . 'conditions' to the normative side." And his view is unequivocal: "In terms of the great dichotomy of this study the social factor becomes a normative, more specifically, a value factor, not one of heredity and environment" (*S*, p. 464)—these being, again, the sole alternatives he sees.²⁵

But even if we bracket this particular view of the social, Parsons's more basic assertion that the categories of action theory subsume all other perspectives on the social world must still be understood, as Bendix and Berger have pointed out, as a move to generalize "yet another selectively perceived 'reality'" (1959, p. 94; also Lockwood 1956, p. 138). However well such a claim may have fitted Parsons's campaign for sociology, and however often it has been seconded by later action theorists, it is a conten-

²⁵ At the start of this paragraph, as elsewhere in this paper, the term "institution" appears, used in the standard dictionary sense of "established social organization." But this usage must not be confounded with Parsons's own, which is of a piece with his conception of the social factor. For Parsons, a "social institution" is a "body of [obligatory] rules . . . derivable from a common value system" (*S*, pp. 297–98, 407–8; 1934a, p. 231; 1935c; for critical discussion, see Mayhew 1984, p. 1294; Rex 1961, p. 112; Sorokin 1947, p. 87).

tion that is no more than asserted in *Structure* and has yet to be established empirically (see Lassman 1980, p. 106; Lidz 1986, p. 164). To the contrary, in the few cases where the action schema has been carefully compared with others, what has emerged is that there are fundamental aspects of social phenomena that elude reduction to the schema's terms. Concerning social relations, for example, the schema has been found to fall short whether "relations" are conceived, in accordance with Simmel, in terms of such transactions as exchange, conflict, and super- and subordination (Levine 1971, p. lvi; 1980, p. lv; 1985 p. 124; Warner 1978, pp. 1346-47); or, in accordance with Marx, as relations of production, particularly class relations (Bhaskar 1979); or, in accordance with Mead, as interpersonal dialogue and interactive communication (Habermas 1981b, pp. 86, 95, 279; also Gouldner 1955, p. 182; Turner 1987). Likewise with regard to social groups and institutions: the analysis of structural variables like size, positional composition, network arrangements, distribution of resources, extent of heterogeneity and differentiation, and patterns of interunit contact has been shown to require more than the "translation" of these phenomena into action terms (see esp. Blau 1977; Levine 1971, pp. lx-lxi; 1985, p. 124). The same is true when the topic is that of social change (Friedrichs 1970, p. 145; Martel 1971, pp. 207-8; Moore 1958, p. 122; Nisbet 1969, pp. 228-34).

But it would be wrong to conclude from this that *Structure* should simply have elevated a schema other than the action frame of reference as the encompassing foundation of sociology. Critics of the book, armed with their alternatives, have sometimes suggested this. But in so doing they capitulate to Parsons's own programmatic logic, to the idea of one unifying framework that can be inserted all over the social map. Such an idea is at variance with the "theoretical pluralism" that is, as Merton has observed, "integral to the socially patterned cognitive processes" of science (1981, p. v; also Bryant 1983; Eckberg and Hill 1979; Levine 1986a; Ritzer 1983, pp. 432-35). Approaches that attempt, following the example of *Structure*, to embrace not only action modes but also institutions, social relations, and the rest with a single theoretical vocabulary impair, as Blau (1987) suggests, the analysis of each domain at an appropriate level and in the terms suited to that level—terms that may take different forms and work for some societies but not all. It is this latter type of analysis, moreover, that makes it possible to consider the interconnection of these phenomena and to do so with reference to different sociohistorical circumstances: to examine, for instance, the conditions under which particular action modes affect, and are affected by, particular kinds of interactional patterns and institutional structures. The protean connection of other social phenomena with the realm of human

action is scarcely at issue, however, when the social world is, as in Parsons's charter, arrogated to action categories from the start.

VII

In defining the unit act, *Structure* makes mention of an agent or actor, a figure about whom the preceding discussion has said little. The omission is not unintentional. According to the book's basic theoretical argument, systems of action are forged (analytically) out of means-end chains rather than out of actors; throughout this argument—and Parsons's striking equational summary of his theory (*S*, pp. 77–84)—the actor all but disappears from view. That this is the case is symptomatic of *Structure's* position regarding the actor.

At one level, the concept of the actor is centrally important to Parsons's project. During the period when he wrote, the concept stood as a significant weapon in the antibehaviorist arsenal. The gestalt psychologist Kurt Koffka expressed this when he observed that a move from "the animal side of man [to] his cultural side" entailed conceptualizing the human "agent" in terms of "the Ego or the Self" (1935, pp. 306–422 [quoting pp. 19, 319]). Parsons, who was impressed with Koffka's work, affirms this view: "The unit of reference which we are considering as the actor is not [the human] organism but an 'ego' or 'self'" (*S*, pp. 47, 745). Unlike Koffka and others, however, Parsons offers no further definition of the actor-ego-self concept and does not even suggest that herein lies a domain of structures and processes of their own "enormous complexity." Determined, rather, to bring as much as possible within the ambit of his social-structural schema and to safeguard this from contemporary psychologism, Parsons tends to treat the actor in the same way he treats social relations and groups, that is, to regard the analysis of the actor, or of the "personality," as requiring simply "another secondary descriptive schema of action" (*S*, p. 741). In the same vein, he reasons that to construct an adequate *explanation* of the actor, one must appeal to the categories of sociology, politics, and economics, supplementing these with psychology only to the extent that there remains a "residuum [in personality] referable to heredity" (*S*, pp. 769–70).

It is as an outgrowth of this orientation that so much of *Structure* "leaves the actor an extremely nebulous, . . . ephemeral abstraction" (Pinney 1940, pp. 185–89; also Sorokin 1966, pp. 413, 417; Turner 1987; Williams 1961, p. 65). The underlying assumptions that served to flesh this abstraction out in Parsons's own thinking surface only in isolated comments, and these pull in two directions. On the one hand, there are passages that advert to the "great importance [of] the biological aspect of

man,” which is rooted in the conditional elements of “heredity and environment” and exhibited in the individual organism’s powers of body and mind, as well as its instincts, drives, and appetitive desires (*S*, pp. 47, 50 n. 3, 296, 402). With no supporting argument, Parsons supposes that these items all bespeak the “unlimited egoism” of the unregulated personality and the fact that “the ‘individual’ element” of the actor is “unmoral chaos” (*S*, pp. 291 n. 1, 385, 402, 436, 692; see also Atkinson 1972, p. 14; Gouldner 1970, pp. 430–32). There are, on the other hand, passages that postulate “that men not only respond to [conditional factors but] try to conform their action to patterns which are, by the actor and other[s], deemed desirable” (*S*, p. 76). In an earlier article, Parsons associated this with “the fact that man is essentially an . . . evaluating creature, [oriented to] ends, purposes, ideals,” and in the book he asserts—without argumentation again—that “man is an entity [inclined] to develop metaphysical interpretations of his world” and that “the feeling of obligation to pursue the good [is] one of the ultimate characteristics of human beings” (1935a, p. 282; *S*, pp. 391 n. 1, 668).

At first glance, these two sets of passages look almost contradictory, but here again the dichotomizing logic of Parsons’s charter must be recalled. During the 1920s and 1930s, it was quite common for proponents of the sociocultural sciences to operate with a bipolar image of personality that represented the actor, to quote Znaniecki (1934, p. 120), as suspended between “psycho-biological characteristics” and a culturally based “system of rights and obligations.” Given his own position in the ongoing debates, Parsons adopts the same bipolar image; his seemingly contrary assumptions about personality are nothing more than corollaries of this. Or, in other words, just as his conceptualizations of action and of social structure prove to be translations of the programmatic dichotomy of his book, so too with his concept of the actor. Despite the availability of alternative conceptions, Parsons casts the actor, directly in terms of the contemporary scene, as an agent inherently pulled between the conditional biological element of egoistic desire and the force of moral obligation that emanates from the normative sociological factor. Happily for students of the latter, moreover, the balance is in their favor. All told, says Parsons, “the normal concrete individual is a morally disciplined personality, [in whom] the normative elements have become internal” (*S*, pp. 385–86).

The emphasis that *Structure* places on this last point has frequently led commentators to charge Parsons with holding an “oversocialized conception of man,” which not only overlooks biological forces and the conflicts produced as these “buck against” societal rules (Wrong 1961, pp. 187–91) but so “blot[s] out the active subject” that the actor becomes merely the

"unthinking dupe of his culture" (Giddens 1976b, pp. 22, 113; also Garfinkel 1967, pp. 66–75; Giddens 1977, p. 167; 1979, pp. 52, 254; Gouldner 1970, p. 206; Heritage 1984, p. 27; Hollis 1977, p. 85). It should now be clear, however, that Parsons's bipolar conception makes way for normative *and* biological personality factors; the former are certainly seen to prevail, but only as an outcome of struggle against "the centrifugal bombardment of . . . appetites" and the "tendency [of] impulses [to escape] normative limitations" (S, p. 685; also Williams 1980, p. 65). The idea that actors are unthinking dupes is, moreover, precisely what Parsons seeks to deny when he conceives actors to be evaluating, metaphysically reflective subjects striving toward the good. On the attack against "anti-intellectualistic theories," he took seriously the research of Wolfgang Köhler that claimed to show that even simple forms of behavior are not "blind" but involve "insight" and "intelligence" (S, p. 5; 1974, p. 56; Köhler 1926, pp. 775–76). In Parsons's view, "passive" conceptions of the actor are the product of the objectionable theories of the biologists, psychologists, and economists, who either limit rationality to successful "adaptation" to conditions or give it up in favor of unreflecting organismic factors; the ultimate safeguard of the "active" conception lies in the place accorded, in his own theory, to value elements and, thereby, to morally obligated, goal-directed, ethically rational conduct (S, pp. 63–64, 115–17, 396–97, 438–39, 685; see also Sec. IX).

These observations suggest that when it is realized that *Structure* adheres to a bipolar model of the actor, its perspective on personality emerges as broader than the literature typically alleges. There remain, however, two intrinsic features of the model that markedly circumscribe its range. The first is that the model knows only the antithesis of the normative and conditional elements; it nowhere allows for personality attributes not resolvable into this highly particular dichotomy. Parsons's analysis marginalizes, for this reason, even the actor's motivation, as both Schutz (1940–41, pp. 32–35) and, later, Parsons himself (1940–41, p. 81; 1951, pp. 8–9, n. 4) have observed. The term "motive" appears in *Structure* only in passing remarks that confound motives either with normatively constituted ends or with egoistic impulses (S, pp. 161–62, 533–34, 636; 1940–41, p. 82; Bierstedt 1938a, 1938b). No space is allotted to the broad array of socially formed motives that underlies conduct in pursuit of normative ends as well as other forms of action and, arguably, militates against the very supposition of egoism (see Camic 1979). The book tends, likewise, to divest actors of their emotions and habits (see Sec. V) and of their social relational and positional properties, that is, of gender, race, age, class, occupation, and the differential possession of cultural capital. Even the cognitive traits that Parsons seeks to preserve fail to

hold their own. In *Structure*, as Warner has demonstrated, the “cognitive factor” is alternately reduced to a “group of elements in the value complex” or equated with verifiable knowledge of “conditions,” a denial in either event of the “analytical independence” of “socially structured cognitive elements” and thus an eclipse of “the cognitive activity and interpretative competence of the actor” (Warner 1978, pp. 1327–33; also *S*, pp. 83, 537; Heritage 1984, pp. 21, 26). These features of Parsons’s analysis, in conjunction with his treatment of motives, emotions, and so forth, vividly attest to how thoroughly the binary terms of his project engulf, and deplete, the concept of personality. In this one respect then, the critics are not entirely wrong when they fault *Structure* for its inactive conception of the actor; for while it is an active conception that Parsons seeks to defend, the impoverishment of the actor that his argument effects leaves the human subject with no real resources—aside, of course, from value factors—with which to bring his or her activity to life.

The second restrictive feature of Parsons’s model is its assumption that the actor is to be viewed chiefly as an agglomerate of elements or factors. Caught up in the campaign to demonstrate that actors respond to normative factors as well as biological forces, Parsons slips over into the view that a collection of diverse elements is what the human personality is. Missing from *Structure* is any appreciation for the alternative idea—available to Parsons in Marshall’s writings on “character” and in Durkheim’s and Weber’s on “habitus”—that personality is less an agglomeration of various elements than the enduring, generalized form, or mold, through which those elements are organized and integrated (though not necessarily in harmonious ways) (see Camic 1986; Heran 1987). Lacking this alternative, the book succumbs to what Sewell (1987, p. 168) describes as the “ahistorical conception of actors.” To Parsons, the characterization of personality as a collection of opposing normative and biological elements is more than a programmatically useful way of speaking of actors amid the controversies of the interwar period: the bipolar image is the general model of personality in all times and places. The only sociohistorical changes that *Structure* can therefore acknowledge with respect to the actor are changes in the content of normative elements or increases in knowledge about the conditions of action (*S*, pp. 144–45, 333, 751–52). Not acknowledged is sociological variation in the underlying form of the human personality itself—and thus in the very manner in which actors emote, cognize and interpret, relate to normative patterns (or biological forces), and so on (see O’Neill 1976, p. 296; Rosaldo 1983). And, as such variation disappears, what necessarily disappears as well is the historical question of the covariation of personality forms with changing social practices, relations, institutions, and the like.

VIII

Abbreviated though it is, Parsons's analysis of the actor enters consequentially into his book's central empirical argument. The fact that the unregulated actor is all egoistic desire immediately raises the prospect of a plurality of such actors coming together to create a momentous "problem of social order." The reasoning here runs along two lines. Focusing on *ends*, Parsons holds—through a logic that critics have rightly found less than airtight (see Barry 1970, pp. 79–80; Camic 1979, pp. 521–22; Warner 1978, pp. 1322–23)—that where there are untamed desires, human ends are "random" and therefore (?) so "diverse [that] there is nothing to prevent their pursuit resulting in conflict" (*S*, p. 89). Turning to *means*, Parsons claims that, given the "scarc[ity] of nonhuman resources" for satisfying desire and that "the services of others [afford further] potential means to . . . ends," egoistic actors will "necessarily . . . seek power over one another" and "endeavor . . . to destroy or subdue [each other] by force or fraud or both," thereby causing a "chaotic and unstable . . . state of war" (*S*, pp. 89–93, 235; also Buxton 1985, pp. 20–23; Warner 1978, pp. 1321–26). The task Parsons sets is the explanation of how "actual society" resolves this problem (*S*, p. 109).

This aspect of his analysis is sometimes regarded as the great achievement of *Structure* (Mitchell 1967, p. 9; Münch 1981, p. 726). It is important to realize, however, that it is an achievement that evidences Parsons's involvement in the contemporary scene. The specific way he presents the problem of order—that is, with the terminology of the action frame of reference and an appeal to the work of Hobbes—was his own, and *Structure* may give aspects of the problem their "most forceful and authoritative formulation" (Giddens 1977, p. 209). The problem itself, though, was a recurrent one among social scientists in the United States of the period who were seeking to identify—through an issue that commanded broad popular interest—the dark spots in behaviorist and orthodox economic theorizing and to establish that their own discipline furnished the explanation of how the problem is solved (see, e.g., Friedrich 1937; MacIver 1931; Znaniecki 1934; also Shils 1963; Wiebe 1967; Wilson 1968).

Parsons's own treatment of the problem of order is similarly infused with interdisciplinary considerations. The problem first becomes prominent in his writing in an early article (1934b, pp. 517–19, 526–27), which seeks to expose the deficiencies in the work of the neoclassicist Lionel Robbins, and the polemical association is retained. In *Structure*, there is scarcely a passage on the problem of order that is not an assertion of the need to move beyond economics, since even a proper analysis of "the economic element" leaves out the mechanism that curtails "force and

fraud" in the marketplace (*S*, p. 236). Nor is this issue resolvable through biology and psychology; for Parsons, these are fields where "random variations" and "random movements" necessarily prevail—with implications of "anarchy" rather than order (*S*, pp. 110–17; cf. Köhler 1926). The discipline of politics is only slightly more relevant. Parsons judges political institutions to have no more than a "secondary importance" in the production of social order, on the one hand because there are ordered societies "entirely without . . . state machinery," on the other hand because state processes may intensify the "struggle of interests" (1934a, p. 230; 1936a, pp. 356–65; *S*, pp. 97 n. 1., 239, 770). This leaves, then, only the common-value elements, upon whose significance the science of sociology is predicated, and it is here that Parsons finds the answer he seeks. Again, his analysis proceeds along two lines. Regarding ends, Parsons maintains that normative factors find "expression" as the ultimate ends of the action process; as such, they (*a*) displace the actor's random, egoistic impulses with those stable, internalized ideals that he or she desires to pursue "for their own sake" and (*b*) supply—since they *are* ends "common to the members of a society," not separate wants that create conflict—the "element of unity [that holds] the whole structure together" (*S*, pp. 167, 247, 249, 260, 385–86, 399). Regarding means, Parsons argues that common values also achieve "embodiment" in stable "system[s] of regulatory rules," which proscribe the use of force and fraud as means to actors' ends; given their basis in ultimate values, such rules exert "moral authority" over actors, who adhere to them out of "obligation" and "respect" (*S*, pp. 313–15, 381–404). *Structure* concludes, therefore, that it is as a result of normative sociological factors that "actual [society is] not a state of war [but] a relatively spontaneous order" (*S*, pp. 92, 362, 386, 392).

The conclusion has been widely challenged. Although some commentators interpret Parsons's concern with the problem of order as evidence that he saw society as a dynamic "powder keg of conflicting forces" (Devereux 1961, p. 33; also Mitchell 1967; Rocher 1975; Sciulli 1984), the majority opinion has been that the early Parsons upheld a static, "overintegrated view of society" that overlooks "matters of . . . conflict and disorder," above all intrasocietal, group-level conflicts about resources, power, and human values themselves (Abercrombie et al. 1980, pp. 48–53, 159; Berger 1962, p. 511; see also Atkinson 1972; Burger 1977; Coser 1956; Ellis 1971; Giddens 1976b, pp. 95–98; Gouldner 1970, 1979; Lockwood 1956; Mayhew 1982, pp. 59–61; Rex 1961, p. 112). But both interpretations require qualification. According to Parsons, "the integration [of society through] a common system of ultimate ends . . . is not a generalized description of the usual concrete state of affairs but formulates only one extreme limiting type" of case (*S*, pp. 248, 263; also

pp. 238, 254–55). This is so because “there is room for wide variation both in the degree of integration and in the kind of [common-value] system”—a circumstance that allows for the “struggle[s] between different individuals and groups for power and wealth” *and* for those “irreconcilable [value] divisions in the social body [that appeared in] the religious wars of the post-Reformation period” (S, pp. 263–64, 434, 643–45; 1934a, p. 231; 1935c, p. 15). Parsons agrees, therefore, with Pareto’s “cyclical theory [of] social change,” according to which periods of social “integration”—when “ideal ends or value elements [exert] effective control . . . over conduct”—undergo “a gradual process of disintegration,” chiefly from “the pressure of appetites and interests” or “the pursuit of wealth and power,” until a “revolution of faith” brings a new end to the “conditions of instability” (S, pp. 284–88, 449–50; see also Buxton 1985, pp. 37–41).

Such passages seem to support the society-as-powder-keg reading of *Structure* over the “overintegrated” alternative. But it is the powder-keg interpretation that is, in fact, the more overstated. This is apparent when one remembers what *Structure* means by power, wealth, and interest. To repeat: for Parsons, power and wealth are instantiations of the broader factor of interest, which, when not a manifestation of value elements, itself derives from the conditional factors of unlimited appetite and desire. His observations on struggles for wealth and power do not, for this reason, open out theoretically into an analysis of the social relational and institutional bases of these struggles and of other tendencies to disintegration and disorder (cf. Dahrendorf 1958). Explaining power and resource struggles continually leads Parsons back, instead, to the *presocial* factor of egoistic human nature, which underpins the “means” and “ends” component of his argument for the Hobbesian problem (see S, pp. 284–88, 298, 402, 465; Warner 1978, pp. 1322–25). Loubser (1976, pp. 4–5) has shown that what the Parsons calls the problem of order is a variety of analytically distinct problems with different sources—economic, political, normative, symbolic, communicational, motivational, temporal. Here too, however, *Structure* suppresses the variety, offering a few comments on normative disunity but otherwise merging all forms of the problem with the conditional factor of unrestrained interests and appetites. This is a universalization of the local again, for Parsons hereby writes large those forces that were the particular preoccupation of his charter, thus replacing the powder keg that economic, political, communicational, motivational, and other forces may make up together with a single explosive—an explosive real enough to prevent perfect integration but still sufficiently delimited to be more or less easily defused.

When this step in Parsons’s reasoning is appreciated, it is possible to understand how *Structure* is also led to dehistoricize the problem of order

and obscure its character as “a concrete historical problem whose terms are defined by . . . the society in which it arises” (Clarke 1982, p. 4). O’Neill sees the particular problem of order at issue in *Structure* as a product of the transformation of “the psychology of individual wants and desires . . . under conditions of the capitalist property system” (1976, pp. 303–6); other scholarship roots the problem in other social and cultural developments in the period from the late 18th century to the early 20th century (see Camic 1979; Clarke 1982; Nisbet 1966; Peel 1971). But no such historical possibility is even raised by Parsons, who postulates that his rendition of the problem identifies a truly “general problem [that requires solution in] every society” (see Bershady 1973, pp. 42–47; Gouldner 1979, p. 299). To temporalize the problem (other than through Pareto’s eternal bust and boom cycles) would be to admit that the empirical issue that occupied the science of sociology lay outside the league of the general problems that occupied the already established sciences.

Similar tendencies toward overgeneralization appear when one turns to Parsons’s discussion of the *solution* of the problem of order. In proposing that order hinges on “the effective functioning of [common] normative elements” (S, p. 92), Parsons presupposes (i) that, in significant measure, common normative elements exist,²⁶ (ii) that these elements translate into the ends and regulatory rules of conduct, and (iii) that actors uphold ends and rules “for their own sake” and out of moral obligation. His analysis grants, as we have seen, that conditional forces may cloud this picture to an extent, but that is the only qualification he makes. For to take any of these three contentions away as a general principle would be to break a necessary link in the whole argument for the normative solution. The scholarly literature (including, in fairness, some of Parsons’s own subsequent work) has shown, however, that none of the three contentions qualifies as a general principle: each identifies only one among several possibilities and not the one most likely. Regarding i, empirical research disconfirms the significant presence of common normative factors not only in certain modern societies, characterized by divergent class, regional, ethnic, and religious subcultures rarely reconciled through higher values, but also in traditional societies, like the Christian Middle Ages—the prototype, for Parsons, of a situation of shared values (see 1935a, p. 296; S, p. 248)—where major segments of the population long remained culturally apart owing to the ineffectiveness of the mechanisms

²⁶ At one point, *Structure* appears to endorse Pareto’s statement that there are “two classes” of ultimate normative factors, “those held distributively by individuals and groups” and “those held in common by the members of the society” (S, p. 297). The book goes on, however, to assign exclusive priority to the second group while allowing the first to drop from sight. In Parsons’s view, “without a system of common values . . . there can be no such thing as a society” (S, p. 434).

for the dissemination of a common cultural code (see Abercrombie et al. 1980; Aberle 1950).

Regarding ii, the assumption that common normative factors translate into regulatory rules and ends has been faulted for failing to recognize (*a*) that the ambiguous, fluid, and often contradictory character of value elements impedes their attaining meaningful embodiment as specific ends and normative rules (see Cancian 1976; Kaplan 1968); (*b*) that even where value factors admit of such embodiment, this may be achieved in moral norms, abstract legal norms, ritual norms, technical standards, standards of public opinion, and elsewhere; the link between any of these domains and those rules that regulate force and fraud and bind actors together in daily conduct cannot be assumed but changes with the cultural integration of these domains and the properties of the normative standards (see Blake and Davis 1964, pp. 461–65; Levine 1968; Sorokin 1947, pp. 83–87); and (*c*) that even where there is a strong link between the value factors and the rules for daily conduct, action process may still remain unregulated in varying degrees owing to the (historically variable) disjunction between normative specifications and the exigencies of concrete practices (see Kaplan 1968, pp. 902–5; Williams 1977, pp. 130–32). Finally, in regard to iii, the message of the scholarship has been that “the extent to which [actors experience and affirm the] oughtness [of normative elements] is an empirical issue, not”—as is often true in *Structure*—a matter “to be decided *a priori*” (Warner 1978, p. 1341). This is so because not all normative factors possess the quality of obligatoriness, because not all actors who internalize formally obligatory norms internalize them *as* obligatory, and because even actors who do may not act (for reasons given in Sec. V) in accord with the normative standards (see Blake and Davis 1964, pp. 464–65; Cancian 1976, pp. 358–59; Holmwood 1983, p. 329; Lockwood 1956, p. 137; Swidler 1986; Warner 1978).

Against these lines of attack, *Structure* has little defense. Although Parsons in his later career conceptualized social psychological and cultural phenomena in ways less open to some of these empirical criticisms, his analysis of them in *Structure* is yet too scanty to lend support to the premises of the normative solution. The closest he comes to furnishing social psychological grounding for the idea that actors follow internalized norms out of obligation is to insert a few unelaborated references to Piaget's work on socialization and Freud's on introjection and to present these as if they applied to all normative elements in all social circumstances (see 1935a, p. 296; S, pp. 386, 388, 401; cf. Hamilton 1983, p. 28). His cultural theory is only slightly more ample. Integral to *Structure*'s assault on the reduction of human action to the terms of the natural sciences is, as noted before, the postulate that humans are “orient[ed] to

the nonempirical aspects of the universe," that is, to a "‘nonempirical reality’ . . . not capable of being scientifically observed" (*S*, pp. 422–23). This established, Parsons, drawing vaguely on Whitehead (esp. 1925, pp. 159–72), conceptualizes "culture" (analytically) as the realm of "eternal objects"—of symbolic, "atemporal complexes of meaning"—that represent attempted "formulations of [this] nonempirical reality," formulations that are necessarily "partial, imperfect, [and] unstable in relation to their referents" (*S*, pp. 449, 482, 763). Geertz (1971, pp. 371–73) judges this view superior to others current in early American social science, and while this may be correct, it is also the case that Parsons’s concept of culture is one singularly ill-fitted to his argument for the normative resolution of the Hobbesian problem. From Whitehead’s diverse, unstable eternal objects to a stable system of shared normative elements is a long distance, which *Structure* does almost nothing to overcome. Indeed, its only bridging device is the fragmented thesis that religious ideas hold an especially central place in the realm of eternal objects, that these ideas are strongly related to a society’s diffuse "ultimate value attitudes," and that such attitudes are themselves interdependent with the society’s established system of shared ultimate values and, thus, with the obligatory normative elements that regulate the means and ends of action (*S*, pp. 255, 267, 424–27; for criticisms, see Alexander 1987b, pp. 29–31; Rex 1961, pp. 91–92; Sorokin 1966, pp. 417–18). That much of this is highly tentative, Parsons acknowledges. So firm, however, is his belief in the normative solution that he is willing to leave its needed cultural underpinnings for clarification at another time and to press on with his conclusion.

And regarding this conclusion, he does not equivocate. Having posed the Hobbesian question as a "general problem," he formulates a resolution that, as Bershady (1973, p. 45) has shown, he likewise renders as the "*general* solution of the problem of order"—one apropos to "actual society," in the sweeping singular (see also Burger 1977). But it is here, perhaps above all, that scholars have taken issue with *Structure*. In accord with their objections to the various steps in its case for the normative solution, the solution itself has been found not to obtain significantly in a diversity of societies, past and present, whose pattern of order—where order is achieved—has been traced far more to economic and political structures and processes, organizational arrangements, forms of intergroup conflict, and so on (see Abercrombie et al. 1980; Alexander 1987b, pp. 30–31; Barry 1970, pp. 86–87; Blau 1977; Cohen 1968; Coser 1956; Ellis 1971; Etzioni 1968; Jessop 1972; Kaplan 1968; Luhmann 1982, p. 74; Warner 1978). Where value factors play an important integrative role, moreover, this has been attributed, in many instances, not to what *Structure* sees as their "spontaneous" ordering effect but mainly to the

American Journal of Sociology

way in which “values are . . . selectively related to extant position-related interests” (Habermas 1981a, p. 179; see also Burger 1977; Ellis 1971; Giddens 1976b; Przeworski 1980).²⁷

In appraising this scholarship, one should beware of critiques that would simply invert Parsons’s approach and transmute various non-normative solutions into a “general” solution. Reflecting on the “historical realities” of ancient Sparta, Japan in 1866, Nazi Germany, and elsewhere, Mills long ago cautioned that “there is no *one* answer to . . . the tired old problem of social order . . . because social structures differ profoundly in their . . . kinds of unity” (1959, pp. 44–47)—a point corroborated by subsequent research on the differential regulative effects of various economic, political, normative, and other factors under different historical circumstances (see esp. Jessop 1972). Parsons, however, again in his eagerness to make sociology a generalizing science with its own domain, eschews such a viewpoint—and not only in its historicist dimension, but even in its concern with the interplay between the normative factors and the more standard economic and political solutions (cf. Burger 1977, p. 326). As one may see from the preceding summary, Parsons’s analysis of these two solutions is an example of “the fallacy of argument by elimination.” It locates aspects of the Hobbesian problem not resolved by market processes and political institutions and leaps to the conclusion that economic and political factors are not “solutions” at all—that they belong instead among the “randomizing,” disordering forces that the value factors are (universally) required to quell rather than (situationally) to complement (Alexander 1983b, pp. 213–14; see also Ellis 1971, p. 693; Williams 1961, p. 66).²⁸ It is interesting that “argument by elimination” was a strategem heavily used by Durkheim, particularly in his efforts to rule out all explanations of social facts aside from his own “sociological” alternative (see Lukes 1973, pp. 31–33), for it is similarly to further the

²⁷ Along similar lines, *Structure* has been attacked for making value elements “the unmoved mover in the theory of action” and failing to appreciate that values are themselves socially produced and sustained (Swidler 1986, p. 274; see also Blake and Davis 1964, p. 463; Giddens 1976b, p. 96; Kolb 1957, p. 116; C’Dea 1976, p. 276; Rocher 1975, p. 62; Scott 1971, p. 9). In fairness, however, it should be noted that, while the book exhibits a tendency in this direction, it does—briefly, to be sure—clearly associate the appearance of “new values” not only with charismatic breakthroughs and the “general effervescence” of social revolutions but also with the application of force (*S*, pp. 291, 450; cf. Alexander 1983b, p. 469; Mayhew 1984, p. 1301; Münch 1981, p. 726).

²⁸ Admittedly, *Structure* allows that a social order, once established, “can remain intact [because] there develops an interlocking of interests in the maintenance of the system.” The book hastens to add, however, that even here “the ultimate source [of order] is the common sense of moral attachment to norms.” Of itself, the “interlocking of interests is a brittle thing which comparatively slight alterations of conditions can shatter at vital points” (*S*, p. 404).

cause of sociology that *Structure* resorts to the tactic. As Parsons sees it, to include economic and political (or other nonsociological) factors in the solution to the problem of order is to minimize the general necessity of the normative element, the cornerstone of sociology, and to merge order with phenomena largely anchored in the conditional.²⁹ Given, though, his desire to extol the normative over the conditional and his belief that conditional elements are what produce disorder, it is a foregone conclusion that *Structure* will find the only genuine solution in the normative realm. In accord with the book's dichotomizing logic, whatever is not part of the required disciplinary solution is part of the problem.

IX

The preceding sections have shown that there is considerable thematic unity in *Structure*. Whether Parsons's immediate topic is social order, personality, the social system, action, the history of social thought, or the scientific method, not only does he focus on an issue relevant to the task of chartering sociology, but he also subjects each to the same polemical categories and universalized formulations so as to reach the same programmatic conclusion. For all this, however, there is a paradoxical tendency for unity on these matters to diminish unity elsewhere in the book. As Parsons mounts his campaign now on this front, now on that, he inevitably molds his message to the topic at hand, thereby producing arguments difficult to reconcile with one another, at least on the surface.

A case in point is "voluntarism," and the situation has wreaked havoc in the literature. According to one school of interpretation, *Structure* offers a major defense of the voluntaristic nature of action, and is right to do so in the judgment of some commentators, wrong to do so in the view of others, all of them further divided over whether Parsons's "voluntarism" stands for choice, freedom of choice, freedom, free will, purposiveness, subjective decision making, subjectivity, activity and creativity in conduct, autonomy from material conditions, or antideterminism (see Adriaansens 1980; Alexander 1978, 1979, 1983b, 1987b; Bershady 1973, 1974; Bourricaud 1977; Buxton 1985; Cohen 1975; Coser 1977; Devereux

²⁹ What Parsons wishes not to merge with conditional factors is "social order" in the sense of a resolution of the Hobbesian state of war. It should be clear from what has been said about the bipolar structure of his entire analysis, however, that Parsons does not attribute the concrete "factual order" of a society, i.e., the pattern of regularities that the scientist may observe in social phenomena, to normative elements alone (on these two meanings of order, see *S*, pp. 91–92). Rather, to the extent that factual order is the issue (albeit only to this extent), one may agree with Adriaansens that Parsons's "answer to the question of order in society [is] an answer in which both conditioning . . . factors and normative subjective factors alike play their part" (1980, p. 50; cf. Alexander 1983b; Münch 1981, 1987).

American Journal of Sociology

1961; Friedrichs 1970; Gerstein 1979; Gouldner 1970; Habermas 1981b; Hamilton 1983; Mayhew 1982; Münch 1981, 1982, 1987; Pope 1973; Savage 1981; Sciulli 1986; Scott 1963; Turner and Beeghley 1974). A second school of interpretation holds the opposite position, however: *Structure* does “not successfully embody [the voluntaristic] perspective” that it nominally embraces but advances a “deterministic” theory that suppresses the “active,” “free,” and “creative character of human action” (quotations adapted from Giddens 1976b, pp. 16–22, 94–96; 1977, p. 167; 1984, p. xxxvii; see also Atkinson 1972; DiTomaso 1982; Turner 1987).

Structure furnishes evidence to support nearly all these readings. This evidence appears in different contexts, however. In sections where the aim is to rescue action from the behaviorist assault, Parsons seeks entirely to affirm, as observed before, the significance of subjectivity, choice, purpose and decision, active agency, and the like; and, in loading on the synonyms, he is generous, too, with expressions such as “freedom of human choice” and “this creative element”—terms he then associates with the “voluntaristic character” or “voluntaristic conception of action” (S, pp. 251–53, 389, 391, 439–40, 446). His language here matches that of other contemporary critiques of behaviorism, whether by gestalt psychologists like Koffka, who wrote of “volition” and “voluntarism” (1935, pp. 416–21), or neoclassicists such as Knight, who dwelt on the actor’s “subjectivity,” “creativity,” and “choice” (1924, pp. 231, 243). What complicates things in Parsons’s case is that he is on the attack not only against behaviorism but also against neoclassicism. For, in those parts of the book where his concern is to indict the economists for neglecting problems of order and random ends and to demonstrate the role of normative factors in resolving these problems, what is instead emphasized is “the enormous empirical importance of coercion in actual economic life” and “human affairs” and the fact that normative elements regulate and “exercis[e] discipline over conduct” and thus “may be held to determine action”—“to be . . . fundamental elements in the determination of men’s action in society” (S, pp. 272, 284, 425–27, 658).

Squaring this talk of determinism with the language of voluntarism is not of primary concern to Parsons. His two vocabularies are not, after all, in direct collision; each appears in a fairly discrete context and works with the other to further the cause of sociology. For the most part, then, ad hoc reconciliations can suffice, and of these he offers two. The first is the quasi-Kantian idea that the discipline and constraint exercised by normative elements take the form of “voluntary adherence to [these elements] as a duty”: “The actor is not free to do as he likes [and] is bound,” but bound by “moral obligations” that he “desires” to fulfill (S, pp. 383–87). The second is an argument that actually turns the focus away from the concrete actor and onto the so-called general analytical level with the claim

that, although the individual's "own concrete ends or other concrete norms . . . are by no means wholly or even substantially his own creation," they are "not beyond the control of human agency in general" (*S*, pp. 370, 709).

What makes these formulations hard to sort out is that *Structure* favors the descriptive mention of voluntarism over the business of formal definition. Significantly, though, there are a few places in the text and in the early articles where Parsons does, nevertheless, affix a *core* meaning to the concept, equating "the voluntaristic character of action" specifically with "the independence of ends and other normative elements of the structure of action from determination in terms of heredity and environment" (*S*, p. 700; also pp. 483–84; 1934*b*, pp. 515–21; 1935*a*, pp. 284–92). This core idea is captured well by Alexander when he interprets Parsons's early voluntarism as "freedom vis-à-vis the conditions of action" (1983*b*, p. 35; also 1978), and one need only substitute for Alexander's notion of material conditions the factors of heredity and environment to arrive at the basic point. This is that Parsons's concept of voluntarism is shot through with the practical message of his charter, just as are his concepts of action, the social, and so forth. Engaged in a self-professed "war of independence" for the science of normative elements against the biological sciences (see Sec. I), he conceptualizes voluntarism itself as the "independence of normative elements from determination" by these same hostile forces. It is important to notice, furthermore, that this conceptualization affords Parsons considerable latitude to make the seemingly divergent statements he does about voluntarism without troubling over their consistency. Following one strand of the conceptualization, for instance, voluntarism emerges not as some unqualified antideterminism or freedom but as the autonomy from conditional forces that is made possible by the value factor, which Parsons describes accordingly as "the creative element in action in general" (1935*a*, p. 306 n. 19; also *S*, pp. 420, 446). Seen from this angle, voluntarism is not at odds with normative interposition but dependent upon it. This is what Parsons apparently has in mind when he affirms the large place of voluntarism with respect to "human agency in general."

But, from the same conceptual point of departure, Parsons goes in other directions as well. With the idea of voluntarism as "independence" from conditions in place, he moves to appropriate whatever other varieties of independence and autonomy serve his purpose, regardless of how well these would otherwise seem to fit together. It is thus that voluntarism comes to envelop freedom of choice, subjectivity, and voluntary obedience, and thus that, in a final major twist, it extends its hold as well to the force of human "effort." This is evident in an important set of passages where voluntarism is actually explicated chiefly with reference to "the

element of effort" that enables action processes to overcome conditional obstacles in the pursuit of normative ideals (*S*, pp. 251–53, 396; also Procter 1978; Savage 1981). Seeking to appear scientific, Parsons deems the notion of effort "closely analogous to [the concept of] energy in physics" (*S*, p. 719). This said, though, he assimilates it also with the term "will" (*S*, pp. 252, 440), which—if one accepts his later remarks on his early years—he associates further with "free will," but does not emphasize the association in deference to advice from L. J. Henderson (see Parsons's statements in Johnson 1975, pp. 2–3; see also *S*, p. 585).

What makes the equation of voluntarism with effort, will, and perhaps even free will especially significant is that it provides one of the strongest reasons that scholars have brought forth for viewing Parsons's book as an expression of his religious upbringing and bourgeois reaction to the chaos of the 1930s (see Sec. I). Creelan (1978, pp. 182–85) believes, for example, that it is *Structure*'s emphasis on will that particularly discloses its roots in the Congregationalist tradition, while Gouldner (1970, pp. 189–95) sees the concern with free will and effort as evidence of Parsons's interest in combating historical materialism by "reassur[ing] men that . . . their efforts make a 'difference.'" Even if warranted, however, these observations do not deny that Parsons also had two fundamental programmatic reasons for linking voluntarism to effort and will. The first was his desire to prevent possible counterattacks on the normative domain by proponents of the conditional. *Structure* raises the concern that, even when the role of the value element is demonstrated, "it may still be possible to think of this element as automatically self-realizing," much like the "automatic processes" of the biological sciences. By stressing the factors of will and effort, which are "far from being automatic," Parsons felt he was delivering a "decisive rejection of this position" (*S*, pp. 251, 440). But his second reason for accenting these factors was of greater importance: will and effort furnished incontrovertible proof of the inadequacy of the various theories of human conduct then available. What the neoclassicists, the behaviorists, and other enemy forces all share, according to Parsons, is the conviction that their own analytical schema is a "deterministic" one, that is, one that in and of itself allows for the "full explanation" of action and requires no theoretical supplementation; yet each schema rests on assumptions that hold only by virtue of "the exercise of will"—to the "laws" of neoclassicism and behaviorism, "there is always [attached] an 'if,' [which] is the hiding place for the freedom of human volition." These theories are incapable, therefore, of providing a full explanation of action, and the door is open to other disciplinary frameworks, not least one that not only makes way for this "voluntaristic" element but also brings it within the purview of science, the argument of *Structure* being that the common value factors that arouse human faith "in turn stimulate will" (*S*,

pp. 29, 88–89, 379, 440 n. 1, 476, 735; 1934b, pp. 539–40; 1935a, pp. 286–87, 303; 1936b, p. 257; and Parsons's late comments on *Structure* in Johnson 1975, pp. 155–58; Parsons 1973–74, pp. 5, 10–11; 1974, pp. 55–56). Sociology offers that framework; will and effort provide its ultimate justification.

Particularly striking in all this is Parsons's presumption that the specific conception of voluntarism that accords with his charter is voluntarism as such. To him, voluntarism, conceived as the independence of normative elements from heredity and environment, with a dose of subjectivity, free choice, and will superadded, is not one in an array of defensible conceptions; it is the only acceptable possibility—never mind all the meanings that terms like voluntarism, independence, and freedom have taken on for philosophers, social thinkers, and the bearers of the world's political, religious, and economic movements. But not only does he privilege his notion of voluntarism; he vastly generalizes its range of empirical applicability. When a contemporary of Parsons's like Koffka spoke of voluntarism, he too privileged a single analogous definition, but with the stipulation that voluntaristic conduct is most “typical of Western civilization” and of certain personality types found therein (1935, pp. 419–20). *Structure* does not say this; it presents voluntarism as (in Alexander's words [1978, p. 184]) “a universal property of all action abstracted from time and space,” not as a trait that sometimes accompanies action and sometimes does not (cf. Sciulli 1986). To see the issue in the latter way is unacceptable, according to Parsons, because it grants the existence of “nonvoluntaristic action,” which—following his dichotomous reasoning—amounts to action that *is* determined by heredity and environment and *is* explicable in “natural science terms” (S, p. 762 n. 1). But this is exactly what Parsons cannot grant without vitiating his rationale for an independent sociology, and he holds firm against it. For him, “nonvoluntaristic action” is a violation of the very definition of action,³⁰ not a point of departure for an analysis of the historical conditions that release or restrict the “voluntaristic character of action.” One might, it is true, extract from some of his statements about the changing content of norms and the increasing knowledge of conditions the suggestion that “independence from conditions” itself undergoes variation over time, but even this exceeds what Parsons actually says. At all events, the idea of historicizing voluntarism—whether in the meaning that he assigns to it or in other meanings that might be assigned—by embedding it in a changing matrix of personality forms, social practices and action modes, social relations,

³⁰ In the same vein, Parsons comments that “the qualifying adjective, ‘voluntaristic’ [is ultimately] superfluous”; “if a theory of action is to have the status of an independent analytical system at all, it must, in the nature of the case, be a voluntaristic theory” (S, p. 762 n. 1).

American Journal of Sociology

interactional patterns, and institutions is an idea that his book simply is theoretically unequipped to pursue.

CONCLUSION

Why still bother with *The Structure of Social Action*? There are obvious reasons: because of the book's large "influence" on the subsequent intellectual history of sociology;³¹ because of the substantive "contribution" it arguably still makes to sociologists' continuing discussion of such topics as the foundations of social order, the linkage between action and social system, the methodology of the social sciences, the meaning of the "classic" texts of Durkheim and Weber; because, like it or not, the book itself is now a classic in the theoretical tradition of sociology—a text to which theorists and students of theory appeal again and again, though generally without coming to terms with the vast body of piecemeal commentary that has accompanied the volume's rise to classic status. But were each of these whiggish reasons to dissolve, another would remain. By virtue of its sweep, its determination to take a single, bold line of argument and to work it out with regard to issue after issue, *Structure* is a great specimen of the theoretical logic of sociology in operation. In coming to understand the book, one comes to understand not only the views that one theorist put forth but also something about the manner in which a theorist reasons, about what underlies a particular form of theoretical reasoning, and about the critical limitations of that form.

When the scholarship on *Structure* is brought together and the book is examined in relation to the sociointellectual setting where it was produced, what emerges is that the volume is less a dispassionate dialogue with the masters of sociology over the timeless questions of social theory (see, *inter alia*, Alexander 1983b) than a deeply felt tract for the times. The analysis has shown that the issues Parsons chooses to address in his book, the way in which he conceptualizes these issues, and the conclusions he reaches in each major area are all fitted directly to his goal of defending the discipline of sociology at a time when its future was in considerable doubt—of supplying a comprehensive charter that would permanently redeem the fledgling science in the face of powerful intellectual and institutional forces either content with its "pariah" status or on

³¹ To date, there has been no systematic research on the book's influence. If one judges from the evidence available, *Structure* was a "sleeper," which was known at first chiefly in the network constituted by Parsons's early students and attained a wide audience only in the 1950s, when Parsons's later books (and positions) brought increased prominence to his work as a whole (see the sources cited on the first page of the article and Alexander 1982a, 1983b; Atkinson 1972; Berger 1962; Boskoff 1950; Friedrichs 1970; Gouldner 1979; Hinkle 1963; House 1949; Mullins 1973).

Structure

the assault against its field of study, the sociocultural realm of subjective purposes, attitudes, and values. *Structure* is the great manifesto for the normative domain and sociology's claim on it. In it, the science of normative elements at once receives a defensible method, a distinguished ancestry, an analytical framework, an appropriate conception of social structure and personality, an urgent empirical problematic, and a proto-philosophical justification.

But the book here reaches its limit, as its very effectiveness as a charter leads it not only to cast each of these issues too exclusively in terms of the dichotomy between normative factors and factors linked to the disciplines that sociology then opposed but also to render the specific formulations forged in the process as the only legitimate possibilities. The lesson of the numerous critiques of *Structure* taken together is that this charter maker's logic is a flawed one, a preemptive suppression of methodological alternatives, intellectual legacies, conceptions of action and social structure and personality, approaches to the causes and solutions of the problem of social order, and perspectives on human voluntarism that, when once subtracted from the field of sociological vision, are no longer readily recoverable. There is irony, of course, to *The Structure of Social Action*. The book is an all-out argument on behalf of sociology, but, as we look back upon it, it offers a theory of action, not a sociology of action concerned with sociohistorical differences in action modes; a discussion of past social theories, not a sociological analysis of the processes by which such theories emerge, grow, and change; a universalized conception of the actor, not a sociological model of historical changes in personality forms; a generalized treatment of the problem of social order, not a sociological examination of the problem's different causes and "solutions" in different times and places; and on it goes. Yet Parsons's sense of the matter was no doubt correct. Recalling the ancient epigram, revived by Isaiah Berlin (1953), that "the fox knows many things, but the hedgehog one big thing," we might classify the author of *Structure* with the latter: a theorist who met the call for a hedgehog—for a generalized defense of a science of normative elements—in a historical setting where foxes, chasing all the differences in the social world, would surely have been routed by larger hedgehogs already on the field. Sociology has not yet outlived its need of hedgehog volumes—but foxes must beware.

REFERENCES

Abercrombie, Nicholas, Stephen Hill, and Bryan S. Turner. 1980. *The Dominant Ideology Thesis*. London: Allen & Unwin.
Aberle, David F. 1950. "Shared Values in Complex Societies." *American Sociological Review* 15:495–502.

American Journal of Sociology

Adriaansens, Hans P. M. 1980. *Talcott Parsons and the Conceptual Dilemma*. London: Routledge & Kegan Paul.

Alexander, Jeffrey C. 1978. "Formal and Substantive Voluntarism in the Work of Talcott Parsons: A Theoretical and Ideological Reinterpretation." *American Sociological Review* 43:177–98.

—. 1979. "Once Again: The Case for Parsons's Voluntarism." *American Sociological Review* 44:175–77.

—. 1982a. *Theoretical Logic in Sociology*. Vol. 1: *Positivism, Presuppositions, and Current Controversies*. Berkeley and Los Angeles: University of California Press.

—. 1982b. *Theoretical Logic in Sociology*. Vol. 2: *The Antinomies of Classical Thought: Marx and Durkheim*. Berkeley and Los Angeles: University of California Press.

—. 1983a. *Theoretical Logic in Sociology*. Vol. 3: *The Classical Attempt at Synthesis: Max Weber*. Berkeley and Los Angeles: University of California Press.

—. 1983b. *Theoretical Logic in Sociology*. Vol. 4: *The Modern Reconstruction of Classical Thought: Talcott Parsons*. Berkeley and Los Angeles: University of California Press.

—. 1987a. "Parsons's *The Structure of Social Action*." Paper presented at the American Sociological Association meetings, Chicago.

—. 1987b. *Twenty Lectures: Sociological Theory since World War II*. New York: Columbia University Press.

Alexander, Jeffrey C., and Bernhard Giesen. 1987. "From Reduction to Linkage: The Long View of the Micro-Macro Link." Pp. 1–42 in *The Micro-Macro Link*, edited by Jeffrey C. Alexander et al. Berkeley and Los Angeles: University of California Press.

Alexander, Jeffrey C., Bernhard Giesen, Richard Münch, and Neil J. Smelser, eds. 1987. *The Micro-Macro Link*. Berkeley and Los Angeles: University of California Press.

Allport, Floyd H. 1927. "The Nature of Institutions." *Social Forces* 6:167–79.

Annan, Noel. 1959. *The Curious Strength of Positivism in English Political Thought*. London: Oxford University Press.

Apter, David E. 1987. "Parsons's Politics." *American Journal of Sociology* 93:451–56.

Atkinson, Dick. 1972. *Orthodox Consensus and Radical Alternative*. New York: Basic.

Bannister, Robert C. 1987. *Sociology and Scientism: The American Quest for Objectivity, 1880–1940*. Chapel Hill: University of North Carolina Press.

Barber, Bernard. 1970. "Introduction." Pp. 1–53 in *L. J. Henderson on the Social System*, edited by Bernard Barber. Chicago: University of Chicago Press.

—. 1985. "Beyond Parsons's Theory of the Professions." Pp. 211–24 in *Neofunctionalism*, edited by Jeffrey C. Alexander. Beverly Hills, Calif.: Sage.

—. 1986. "Theory and Fact in the Work of Talcott Parsons." Pp. 123–30 in *The Nationalization of the Social Sciences*, edited by Samuel Z. Klausner and Victor M. Lidz. Philadelphia: University of Pennsylvania Press.

—. 1987. "Introductory Remarks by Bernard Barber for 1987 ASA Session on 50th Anniversary of Parsons' *The Structure of Social Action*." Paper presented at the American Sociological Association meetings, Chicago.

Barry, Brian. (1970) 1978. *Sociologists, Economists, and Democracy*. Chicago: University of Chicago Press.

Becker, Howard. 1950. *Through Values to Social Interpretation*. Durham, N.C.: Duke University Press.

Bendix, Reinhard. 1971. "Two Sociological Traditions." Pp. 282–98 in *Scholarship and Partisanship*, by Reinhard Bendix and Guenther Roth. Berkeley: University of California Press.

Bendix, Reinhard, and Bennett Berger. 1959. "Images of Society and Problems of Concept Formation in Sociology." Pp. 92–118 in *Symposium on Sociological Theory*, edited by Llewellyn Gross. New York: Harper.

Berger, Bennett M. 1962. "On Talcott Parsons." *Commentary* 34 (6): 507–13.

Berger, Peter L., and Thomas Luckmann. (1966) 1967. *The Social Construction of Reality*. New York: Doubleday.

Berlin, Isaiah. (1953) 1978. "The Hedgehog and the Fox." Pp. 22–81 in *Russian Thinkers*, by Isaiah Berlin. New York: Viking.

Bershady, Harold J. 1973. *Ideology and Social Knowledge*. Oxford: Blackwell.

_____. 1974. "Commentary on Talcott Parsons' Review of H. J. Bershady's *Ideology and Social Knowledge*." *Sociological Inquiry* 44:279–82.

Bhaskar, Roy. 1979. "On the Possibility of Social Scientific Knowledge and the Limits of Naturalism." Pp. 107–39 in *Issues in Marxist Philosophy*. Vol. 3: *Epistemology, Science, Ideology*, edited by John Mepham and David-Hillel Ruben. Sussex: Harvester.

Bierstedt, Robert. 1938a. "Is Homo Sapient?" *Saturday Review*, March 12, pp. 18–19.

_____. 1938b. "The Means-End Schema in Sociological Theory." *American Sociological Review* 3:665–71.

_____. 1981. *American Sociological Theory*. New York: Academic.

Black, Max. (1961) 1976. "Some Questions about Parsons' Theories." Pp. 268–88 in *The Social Theories of Talcott Parsons*, edited by Max Black. Carbondale: Southern Illinois University Press.

Blake, Judith, and Kingsley Davis. 1964. "Norms, Values, and Sanctions." Pp. 456–84 in *Handbook of Modern Sociology*, edited by Robert E. L. Faris. Chicago: Rand McNally.

Blau, Peter M. 1977. "A Macrosociological Theory of Social Structure." *American Journal of Sociology* 83:26–54.

_____. 1987. "Contrasting Theoretical Perspectives." Pp. 71–85 in *The Micro-Macro Link*, edited by Jeffrey C. Alexander et al. Berkeley and Los Angeles: University of California Press.

Blaug, M. 1962. *Economic Theory in Retrospect*. Homewood, Ill.: Irwin.

Blumer, Herbert. 1969. *Symbolic Interactionism: Perspective and Method*. Englewood Cliffs, N.J.: Prentice-Hall.

Boskoff, Alvin. 1950. "The Systematic Sociology of Talcott Parsons." *Social Forces* 28:393–400.

Bottomore, Tom. 1969. "Out of This World." *New York Review of Books*, November 6, pp. 34–39.

Bourriaud, François. (1977) 1981. *The Sociology of Talcott Parsons*, translated by Arthur Goldhammer. Chicago: University of Chicago Press.

Braudel, Fernand. (1979) 1981. *The Structures of Everyday Life*, translated by Sian Reynolds. New York: Harper & Row.

Brinton, Crane. 1939. "What's the Matter with Sociology." *Saturday Review of Literature* 20 (2): 3–4, 14.

Bryant, Christopher G. A. 1983. "Who Now Reads Parsons?" *Sociological Review*, n.s., 31:337–49.

Burger, Thomas. 1976. *Max Weber's Theory of Concept Formation*. Durham, N.C.: Duke University Press.

_____. 1977. "Talcott Parsons, the Problem of Order in Society, and the Program of an Analytical Sociology." *American Journal of Sociology* 83:320–34.

Burrow, J. W. 1966. *Evolution and Society*. Cambridge: Cambridge University Press.

Buxton, William. 1985. *Talcott Parsons and the Capitalist Nation-State*. Toronto: University of Toronto Press.

American Journal of Sociology

Camfield, Thomas M. 1973. "The Professionalization of American Psychology, 1870–1917." *Journal of the History of the Behavioral Sciences* 9:66–75.

Camic, Charles. 1979. "The Utilitarians Revisited." *American Journal of Sociology* 85:515–50.

_____. 1986. "The Matter of Habit." *American Journal of Sociology* 91:1039–87.

_____. 1987. "The Making of a Method." *American Sociological Review* 52:421–39.

Cancian, Francesca. 1976. "Norms and Behavior." Pp. 354–66 in *Explorations in General Theory in Social Science*, vol. 1. Edited by Jan J. Loubser, Rainer C. Baum, Andrew Effrat, and Victor Meyer Lidz. New York: Free Press.

Catlin, George. 1939. Review of *The Structure of Social Action*, by Talcott Parsons. *Political Science Quarterly* 54:264–66.

Church, Robert L. 1965. "The Economists Study Society: Sociology at Harvard, 1891–1902." Pp. 18–90 in *Social Sciences at Harvard, 1860–1920*, edited by Paul Buck. Cambridge: Harvard University Press.

Clarke, Simon. 1982. *Marx, Marginalism and Modern Sociology: From Adam Smith to Max Weber*. London: Macmillan.

Cohen, Jere. 1975. "Moral Freedom through Understanding in Durkheim." *American Sociological Review* 40:104–6.

Cohen, Jere, Lawrence E. Hazelrigg, and Whitney Pope. 1975a. "De-Parsonizing Weber: A Critique of Parsons' Interpretation of Weber's Sociology." *American Sociological Review* 40:229–41.

_____. 1975b. "Reply to Parsons." *American Sociological Review* 40:670–74.

Cohen, Percy. 1968. *Modern Social Theory*. London: Heinemann Educational.

Coleman, James S. 1986. "Social Theory, Social Research, and a Theory of Action." *American Journal of Sociology* 91:1309–35.

Collini, Stefan, Donald Winch, and John Burrow. 1983. *That Noble Science of Politics*. Cambridge: Cambridge University Press.

Collins, Randall. 1968. "A Comparative Approach to Political Sociology." Pp. 42–67 in *State and Society*, edited by Reinhard Bendix. Boston: Little, Brown.

_____. 1985. *Three Sociological Traditions*. New York: Oxford University Press.

Coser, Lewis. 1956. *The Functions of Social Conflict*. New York: Free Press.

_____. 1977. *Masters of Sociological Thought*, 2d ed. New York: Harcourt Brace Jovanovich.

Cravens, Hamilton. 1978. *The Triumph of Evolution: American Scientists and the Heredity-Environment Controversy, 1900–1941*. Philadelphia: University of Pennsylvania Press.

Cravens, Hamilton, and John C. Burnham. 1971. "Psychology and Evolutionary Naturalism in American Thought, 1890–1940." *American Quarterly* 23:635–57.

Crawford, W. Rex. 1938. Review of *The Structure of Social Action*, by Talcott Parsons. *Annals of the American Academy* 198:178–79.

Creelan, Paul G. 1978. "Social Theory as Confession: Parsonian Sociology and the Symbolism of Evil." Pp. 173–96 in *Structure, Consciousness and History*, edited by Richard Harvey Brown and Stanford M. Lyman. Cambridge: Cambridge University Press.

Curti, Merle. 1980. *Human Nature in American Thought*. Madison: University of Wisconsin Press.

Dahrendorf, Ralf. 1958. "Out of Utopia: Toward a Reorientation of Sociological Analysis." *American Journal of Sociology* 64:115–27.

Devereux, Edward C., Jr. (1961) 1976. "Parsons' Sociological Theory." Pp. 1–63 in *The Social Theories of Talcott Parsons*, edited by Max Black. Carbondale: Southern Illinois University Press.

DiTomaso, Nancy. 1982. "Sociological Reductionism from Parsons to Althusser: Linking Action and Structure in Social Theory." *American Sociological Review* 47:14–28.

Eckberg, Douglas Lee, and Lester Hill, Jr. 1979. "The Paradigm Concept and Sociology: A Critical Review." *American Sociological Review* 44:925–37.

Eisenstadt, S. N., with M. Curelaru. 1976. *The Form of Sociology—Paradigms and Crises*. New York: Wiley.

Ellis, Desmond P. 1971. "The Hobbesian Problem of Order: A Critical Appraisal of the Normative Solution." *American Sociological Review* 36:692–703.

Etzioni, Amitai. 1968. *The Active Society*. New York: Free Press.

Faris, Ellsworth. 1953. Review of *The Social System*, by Talcott Parsons. *American Sociological Review* 18:103–6.

Fearing, Franklin. 1930. *Reflex Action: A Study in the History of Physiological Psychology*. Baltimore: Williams & Wilkins.

Finer, S. E. 1966. "Introduction." Pp. 3–91 in *Vilfredo Pareto: Sociological Writings*, edited by S. E. Finer. London: Pall Mall.

Ford, Joseph Brandon. 1974. "Parsons versus Comte: On Positivism." *Indian Journal of Sociology* 15:77–100.

Foss, Daniel. 1963. "The World View of Talcott Parsons." Pp. 96–126 in *Sociology on Trial*, edited by Maurice Stein and Arthur Vidich. Englewood Cliffs, N.J.: Prentice-Hall.

Friedrich, Carl Joachim. 1937. *Constitutional Government and Politics*. New York: Harper.

Friedrichs, Robert W. (1970) 1972. *A Sociology of Sociology*. New York: Free Press.

Garfinkel, Harold. 1967. *Studies in Ethnomethodology*. Englewood Cliffs, N.J.: Prentice-Hall.

—. 1987. "Parsons's *The Structure of Social Action*." Paper presented at the American Sociological Association meetings, Chicago.

Geertz, Clifford. 1971. "After the Revolution: The Fate of Nationalism in the New States." Pp. 357–76 in *Stability and Social Change*, edited by Bernard Barber and Alex Inkeles. Boston: Little, Brown.

Gerstein, Dean Robert. 1979. "Durkheim and *The Structure of Social Action*." *Sociological Inquiry* 49:27–39.

—. 1983. "Durkheim's Paradigm: Reconstructing a Social Theory." Pp. 234–58 in *Sociological Theory, 1983*, edited by Randall Collins. San Francisco: Jossey-Bass.

Gettys, Warner E. 1939. "A New Approach to Social Action." *Social Forces* 17:425–28.

Giddens, Anthony. 1971. *Capitalism and Modern Social Theory*. Cambridge: Cambridge University Press.

—. 1972. "Introduction: Durkheim's Writings in Sociology and Social Philosophy." Pp. 1–50 in *Emile Durkheim: Selected Writings*, edited by Anthony Giddens. Cambridge: Cambridge University Press.

—. 1974. "Introduction." Pp. 1–22 in *Positivism and Sociology*, edited by Anthony Giddens. London: Heinemann.

—. 1976a. "Classical Social Theory and the Origins of Modern Sociology." *American Journal of Sociology* 81:703–29.

—. 1976b. *New Rules of Sociological Method*. New York: Basic.

—. 1977. *Studies in Social and Political Theory*. New York: Basic.

—. 1979. *Central Problems in Social Theory*. Berkeley and Los Angeles: University of California Press.

—. 1982. *Profiles and Critiques in Social Theory*. Berkeley and Los Angeles: University of California Press.

—. 1984. *The Constitution of Society*. Berkeley and Los Angeles: University of California Press.

—. 1987. "Weber and Durkheim: Coincidence and Divergence." Pp. 182–89 in *Max Weber and His Contemporaries*, edited by Wolfgang J. Mommsen and Jurgen Osterhammel. London: Allen & Unwin.

American Journal of Sociology

Gould, Mark. 1981. "Parsons versus Marx: 'An earnest warning. . . .'" *Sociological Inquiry* 51:197–218.

—. 1987. "Voluntarism ≠ Utilitarianism." Unpublished manuscript. Haverford College, Department of Sociology.

Gouldner, Alvin W. (1955) 1973. "Some Observations on Systematic Theory, 1945–55." Pp. 173–89 in *For Sociology*, by Alvin W. Gouldner. New York: Basic.

—. (1970) 1971. *The Coming Crisis of Western Sociology*. New York: Avon.

—. 1979. "Talcott Parsons, 1902–1979." *Theory and Society* 8:299–302.

Habermas, Jürgen. 1981a. "Talcott Parsons: Problems of Theory Construction." *Sociological Inquiry* 51:173–96.

—. (1981b) 1984. *The Theory of Communicative Action*. Vol. 1: *Reason and the Rationalization of Society*. Translated by Thomas McCarthy. Boston: Beacon.

Hall, John R. 1984. "The Problem of Epistemology in the Social Action Perspective." Pp. 253–89 in *Sociological Theory, 1984*, edited by Randall Collins. San Francisco: Jossey-Bass.

Hamilton, Peter. 1983. *Talcott Parsons*. London: Tavistock.

—. 1985. "Introduction." Pp. 8–17 in *Readings from Talcott Parsons*, edited by Peter Hamilton. London: Tavistock.

Hart, Albert Bushnell. 1930. "Government." Pp. 178–86 in *The Development of Harvard University*, edited by Samuel Eliot Morison. Cambridge: Harvard University Press.

Hawthorn, Geoffrey. 1976. *Enlightenment and Despair: A History of Sociology*. Cambridge: Cambridge University Press.

Heran, François. 1987. "La seconde nature de l'habitus." *Revue Française de Sociologie* 28:385–416.

Heritage, John. 1984. *Garfinkel and Ethnomethodology*. Cambridge: Polity.

Hinkle, Roscoe C. 1963. "Antecedents of the Action Orientation in American Sociology before 1935." *American Sociological Review* 28:705–15.

—. 1980. *Founding Theory of American Sociology, 1881–1915*. Boston: Routledge & Kegan Paul.

Hollis, Martin. 1977. *Models of Man*. Cambridge: Cambridge University Press.

Holmwood, John M. 1983. "Action, System and Norm in the Action Frame of References: Talcott Parsons and the Critics." *Sociological Review* 31:310–36.

Holton, Robert J., and Bryan S. Turner. 1986. *Talcott Parsons on Economy and Society*. London: Routledge & Kegan Paul.

Homans, George Casper. 1962. *Sentiments and Activities*. New York: Free Press.

—. 1964. "Bringing Men Back In." *American Sociological Review* 29:809–18.

Horowitz, Irving Louis. 1964. "Max Weber and the Spirit of American Sociology." *Sociological Quarterly* 5:344–54.

House, Floyd N. 1939. Review of *The Structure of Social Action*, by Talcott Parsons. *American Journal of Sociology* 45:129–30.

—. 1949. Review of *The Structure of Social Action*, 2d ed. By Talcott Parsons. *American Journal of Sociology* 55:504–5.

Hughes, H. Stuart. 1958. *Consciousness and Society: The Reorientation of European Social Thought, 1840–1930*. New York: Vintage.

Janowitz, Morris. 1970. "Foreword." Pp. vii–xii in *Chicago Sociology, 1920–1932*, by Robert E. L. Faris. Chicago: University of Chicago Press.

—. 1975. "Sociological Theory and Social Control." *American Journal of Sociology* 81:82–108.

Jessop, Bob. 1972. *Social Order, Reform and Revolution*. London: Macmillan.

Johnson, Barclay D. 1975. "Some Philosophical Problems in Parsons' Early Thought." Ph.D. dissertation. University of California, Berkeley, Department of Sociology.

Johnson, Harry M. 1981. "Foreword." Pp. vii–xv in *The Sociology of Talcott Parsons*, by François Bourricaud. Chicago: University of Chicago Press.

Johnson, Terry, Christopher Dandeker, and Clive Ashworth. 1984. *The Structure of Social Theory*. London: Macmillan.

Jones, Robert Alun. 1977. "On Understanding a Sociological Classic." *American Journal of Sociology* 83:279–319.

———. 1983. "The New History of Sociology." *Annual Review of Sociology* 9:447–69.

———. 1986. "Durkheim, Frazer, and Smith: The Role of Analogies and Exemplars in the Development of Durkheim's Sociology of Religion." *American Journal of Sociology* 92:596–627.

Kaplan, Harold. 1968. "The Parsonian Image of Social Structure and Its Relevance for Political Science." *Journal of Politics* 30:885–909.

Keat, Russell, and John Urry. 1975. *Social Theory as Science*. London: Routledge & Kegan Paul.

Kemper, Theodore D. 1981. "Social Constructionist and Positivist Approaches to the Sociology of Emotions." *American Journal of Sociology* 87:336–62.

Kirkpatrick, Clifford. 1938. Review of *The Structure of Social Action*, by Talcott Parsons. *Journal of Political Economy* 46:588–89.

Knight, Frank H. (1921) 1964. *Risk, Uncertainty and Profit*. New York: Kelley.

———. (1924) 1935. "The Limitations of Scientific Method in Economics." Pp. 229–67 in *The Trend of Economics*, edited by Rexford Guy Tugwell. New York: Crofts.

———. (1925) 1935. "Economic Psychology and the Value Problem." Pp. 76–104 in *The Ethics of Competition*, by Frank H. Knight. New York: Harper.

———. 1928. "Homan's Contemporary Economic Thought." *Quarterly Journal of Economics* 43:132–41.

———. (1933) 1964. "Preface to the Re-issue." Pp. xi–xxxvi in *Risk, Uncertainty and Profit*, by Frank H. Knight. New York: Kelley.

———. 1940. "Professor Parsons on Economic Motivation." *Canadian Journal of Economic and Political Science* 6:460–65.

Koffka, Kurt. 1935. *Principles of Gestalt Psychology*. New York: Harcourt, Brace.

Köhler, Wolfgang. (1926) 1961. "On Insight." Pp. 772–76 in *Theories of Society*, edited by Talcott Parsons et al. New York: Free Press.

Kolb, William L. (1957) 1966. "The Changing Prominence of Values in Modern Sociological Theory." Pp. 93–132 in *Modern Sociological Theory*, edited by Howard Becker and Alvin Boskoff. New York: Holt, Rinehart & Winston.

Lassman, Peter. 1980. "Value-Relations and General Theory: Parsons' Critique of Weber." *Zeitschrift für Soziologie* 9:100–111.

Levine, Donald N. 1968. "Cultural Integration." Pp. 372–80 in *International Encyclopedia of the Social Sciences*, vol. 7. Edited by David Sills. New York: Macmillan.

———. 1971. "Introduction." Pp. ix–lxv in *George Simmel on Individuality and Social Forms*, edited by Donald N. Levine. Chicago: University of Chicago Press.

———. 1980. "Introduction to the Arno Press Edition." Pp. iii–lxix in *Simmel and Parsons: Two Approaches to the Study of Society*, by Donald Nathan Levine. New York: Arno.

———. 1985. *The Flight from Ambiguity*. Chicago: University of Chicago Press.

———. 1986a. "The Forms and Functions of Social Knowledge." Pp. 271–83 in *Metatheory in Social Science*, edited by Donald W. Fiske and Richard A. Schweder. Chicago: University of Chicago Press.

———. 1986b. Review of *Theoretical Logic in Sociology*, vol. 3. By Jeffrey C. Alexander. *American Journal of Sociology* 91:1237–39.

Lidz, Victor. 1986. "Parsons and Empirical Sociology." Pp. 141–82 in *The Nationalization of the Social Sciences*, edited by Samuel Z. Klausner and Victor M. Lidz. Philadelphia: University of Pennsylvania Press.

Lipset, Seymour Martin, and Everett Carll Ladd, Jr. 1972. "The Politics of American Sociologists." *American Journal of Sociology* 78:67–104.

American Journal of Sociology

Lockwood, David. 1956. "Some Remarks on 'The Social System.'" *British Journal of Sociology* 7:134–46.

Lopreato, Joseph. 1971. "The Concept of Equilibrium: Sociological Tantalizer." Pp. 309–43 in *Institutions and Social Exchange*, edited by Herman Turk and Richard L. Simpson. Indianapolis: Bobbs-Merrill.

—. 1980. "Introduction." Pp. xiii–xliv in *Compendium of General Sociology*, by Vilfredo Pareto. Minneapolis: University of Minnesota Press.

Loubser, Jan J. 1976. "General Introduction." Pp. 1–23 in *Exploration in General Theory in Social Science*, vol. 1. Edited by Jan J. Loubser, Rainer C. Baum, Andrew Efrat, and Victor Meyer Lidz. New York: Free Press.

Lowie, Robert H. 1924. *Primitive Religion*. New York: Boni & Liveright.

Luhmann, Niklas. 1982. *The Differentiation of Society*, translated by Stephen Holmes and Charles Larmore. New York: Columbia University Press.

Lukes, Steven. 1973. *Emile Durkheim*. Harmondsworth: Penguin.

Lundberg, George A. 1956. "Some Convergences in Sociological Theory." *American Journal of Sociology* 62:21–27.

MacIver, R. M. (1931) 1936. *Society*. New York: Farrar & Rinehart.

Martel, Martin U. 1971. "Academentia Praecox: The Aims, Merits, and Scope of Parsons' Multisystemic Language Rebellion (1958–1968)." Pp. 175–211 in *Institutions and Social Exchange*, edited by Herman Turk and Richard L. Simpson. Indianapolis: Bobbs-Merrill.

—. 1979. "Talcott Parsons." Pp. 609–30 in *International Encyclopedia of the Social Sciences*. Vol. 18: *Biographical Supplement*. Edited by David L. Sills. New York: Macmillan.

Martindale, Don. 1959. "Talcott Parsons' Theoretical Metamorphosis from Social Behaviorism to Macrofunctionalism." *Alpha Kappa Delta* 29:38–46.

Mayhew, Leon H. 1982. "Introduction." Pp. 1–62 in *Talcott Parsons on Institutions and Social Evolution*, edited by Leon H. Mayhew. Chicago: University of Chicago Press.

—. 1984. "In Defense of Modernity: Talcott Parsons and the Utilitarian Tradition." *American Journal of Sociology* 89:1273–1305.

Menzies, Ken. 1976. *Talcott Parsons and the Social Image of Man*. London: Routledge & Kegan Paul.

Merton, Robert K. 1981. "Foreword: Remarks on Theoretical Pluralism." Pp. i–vii in *Continuities in Structural Inquiry*, edited by Peter M. Blau and Robert K. Merton. Beverly Hills, Calif.: Sage.

Meyer, J. H. 1970. "The Charter: Conditions of Diffuse Socialization in Schools." Pp. 564–78 in *Social Processes and Social Structures*, edited by W. Richard Scott. New York: Holt, Rinehart & Winston.

Mills, C. Wright. (1959) 1961. *The Sociological Imagination*. New York: Grove.

Mitchell, Wesley C. 1910a. "The Rationality of Economic Activity. I." *Journal of Political Economy* 18:97–113.

—. 1910b. "The Rationality of Economic Activity. II." *Journal of Political Economy* 18:197–216.

Mitchell, William. 1967. *Sociological Analysis and Politics: The Theories of Talcott Parsons*. Englewood Cliffs, N.J.: Prentice-Hall.

Moore, Barrington, Jr. 1958. *Political Power and Social Theory*. Cambridge: Harvard University Press.

Mulkay, M. J. 1971. *Functionalism, Exchange and Theoretical Strategy*. London: Routledge & Kegan Paul.

Mullins, Nicholas C. 1973. *Theories and Theory Groups in Contemporary American Sociology*. New York: Harper & Row.

Münch, Richard. 1981. "Talcott Parsons and the Theory of Action. I. The Structure of the Kantian Core." *American Journal of Sociology* 86:709–39.

_____. 1982. "Talcott Parsons and the Theory of Action. II. The Continuity of Development." *American Journal of Sociology* 87:771–826.

_____. 1987. *Theory of Action*. London: Routledge & Kegan Paul.

Nisbet, Robert A. 1966. *The Sociological Tradition*. New York: Basic.

_____. 1969. *Social Change and History*. New York: Oxford University Press.

Oberschall, Anthony. 1972. "The Institutionalization of American Sociology." Pp. 187–251 in *The Establishment of Empirical Sociology*, edited by Anthony Oberschall. New York: Harper & Row.

O'Dea, Thomas F. 1976. "The 'Emergence—Constitution Process' and the Theory of Action." Pp. 277–94 in *Explorations in General Theory in Social Science*, vol. 1. Edited by Jan J. Loubser, Rainer C. Baum, Andrew Effrat, and Victor Meyer Lidz. New York: Free Press.

O'Neill, John. 1976. "The Hobbesian Problem in Marx and Parsons." Pp. 295–308 in *Explorations in General Theory in Social Science*, vol. 1. Edited by Jan J. Loubser, Rainer C. Baum, Andrew Effrat, and Victor Meyer Lidz. New York: Free Press.

Parrish, John B. 1967. "Rise of Economics as an Academic Discipline: The Formative Years to 1900." *Southern Economic Journal* 34:1–16.

Parsons, Talcott. 1934a. "Society." Pp. 225–31 in *Encyclopedia of the Social Sciences*, vol. 14. Edited by Edwin Seligman. New York: Macmillan.

_____. 1934b. "Some Reflections on 'The Nature of Significance of Economics.'" *Quarterly Journal of Economics* 48:511–45.

_____. 1935a. "The Place of Ultimate Values in Sociological Theory." *International Journal of Ethics* 45:282–316.

_____. 1935b. "Sociological Elements in Economic Thought. I." *Quarterly Journal of Economics* 49:414–53.

_____. 1935c. "Prolegomena to a Theory of Social Institutions." Talcott Parsons Papers. Harvard University Archives, Cambridge. (Date approximate.)

_____. 1936a. "On Certain Sociological Elements in Professor Taussig's Thought." Pp. 359–79 in *Explorations in Economics: Notes and Essays Contributed in the Honor of F. M. Taussig*, edited by Jacob Viner. New York: McGraw-Hill.

_____. 1936b. "Pareto's Central Analytical Scheme." *Journal of Social Philosophy* 1:244–62.

_____. (1937a, 2d ed. 1949) 1968. *The Structure of Social Action*. New York: Free Press.

_____. (1937b) 1938. "The Role of Theory in Social Research." *American Sociological Review* 3:13–20.

_____. (1940–41) 1978. Letters to Alfred Schutz. Pp. 3, 63–70, 72–93, and 107–9 in *The Theory of Social Action*, edited by Richard Garthoff. Bloomington: Indiana University Press.

_____. 1951. *The Social System*. New York: Free Press.

_____. (1957) 1977. "Malinowski and the Theory of Social Systems." Pp. 82–99 in *Social Systems and the Evolution of Action Theory*, by Talcott Parsons. New York: Free Press.

_____. 1959a. "An Approach to Psychological Theory in Terms of the Theory of Action." Pp. 612–711 in *Psychology: A Study of a Science*, vol. 3. Edited by Sigmund Koch. New York: McGraw-Hill.

_____. 1959b. "Short Account of My Intellectual Development." *Alpha Kappa Delta* 29:3–12.

_____. (1961) 1976. "The Point of View of the Author." Pp. 311–63 in *The Social Theories of Talcott Parsons*, edited by Max Black. Carbondale: Southern Illinois University Press.

_____. 1968. "Introduction to the Paperback Edition." Pp. v–xiv in *The Structure of Social Action*, by Talcott Parsons. New York: Free Press.

American Journal of Sociology

_____. 1969a. "Author's Introduction." Pp. 59–63 in *Politics and Social Structure*, by Talcott Parsons. New York: Free Press.

_____. 1969b. "Introduction." Pp. xi–xvii in *Politics and Social Structure*, by Talcott Parsons. New York: Free Press.

_____. 1970. "On Building Social Systems Theory: A Personal History." *Daedalus* 99:826–81.

_____. 1971. "Higher Education as a Theoretical Focus." Pp. 233–52 in *Institutions and Social Exchange*, edited by Herman Turk and Richard L. Simpson. Indianapolis: Bobbs-Merrill.

_____. (1973–74) n.d. "Dialogues with Parsons," edited by Martin V. Martel. Pp. 1–34 in *Essays on the Sociology of Talcott Parsons*, edited by G. C. Hallen. Meerut, India: Indian Journal of Social Research.

_____. 1974. "Comment on: 'Current Folklore in the Criticisms of Parsonian Action Theory.'" *Sociological Inquiry* 44:55–58.

_____. 1975a. "Comment on 'Parsons' Interpretation of Durkheim' and on 'Moral Freedom through Understanding in Durkheim.'" *American Sociological Review* 40:106–11.

_____. 1975b. "On 'De-Parsonizing' Weber." *American Sociological Review* 40:666–70.

_____. 1976a. "Clarence Ayres's Economics and Sociology." Pp. 175–79 in *Science and Ceremony*, edited by William Breit and William Patton Culbertson, Jr. Austin: University of Texas Press.

_____. 1976b. "Reply to Cohen, Hazelrigg and Pope." *American Sociological Review* 41:361–65.

_____. 1977. "Comment on Burger's Critique." *American Journal of Sociology* 83:335–39.

_____. 1978. "Comment on R. Stephen Warner's 'Toward a Redefinition of Action Theory: Paying the Cognitive Element Its Due.'" *American Journal of Sociology* 83:1350–58.

Peel, J. D. Y. 1971. *Herbert Spencer: The Evolution of a Sociologist*. London: Heinemann.

Perrin, Robert G. 1976. "Herbert Spencer's Four Theories of Social Evolution." *American Journal of Sociology* 81:1339–59.

Pinney, Harvey. 1940. "The Structure of Social Action." *Ethics* 50:164–92.

Pope, Whitney. 1973. "Classic on Classic: Parsons' Interpretation of Durkheim." *American Sociological Review* 38:399–415.

_____. 1976. *Durkheim's Suicide: A Classic Reanalyzed*. Chicago: University of Chicago Press.

Pope, Whitney, Jere Cohen, and Lawrence E. Hazelrigg. 1975. "On the Divergence of Weber and Durkheim: A Critique of Parsons' Convergence Thesis." *American Sociological Review* 40:417–27.

_____. 1977. "Reply to Parsons." *American Sociological Review* 42:804–11.

Procter, Ian. 1978. "Parsons's Early Voluntarism." *Sociological Inquiry* 48:37–48.

_____. 1980. "Voluntarism and Structural-Functionalism in Parsons' Early Work." *Human Studies* 3:331–46.

Przeworski, Adam. 1980. "Material Bases of Consent: Economics and Politics in a Hegemonic System." *Political Power and Social Theory* 1:21–66.

Pursell, G. (1958) 1982. "Unity in the Thought of Alfred Marshall." Pp. 287–99 in *Alfred Marshall: Critical Assessments*, vol. 1. Edited by John Wood Cunningham. London: Croom Helm.

Rex, John. 1961. *Key Problems of Sociological Theory*. London: Routledge & Kegan Paul.

Ritzer, George. 1983. *Sociological Theory*. New York: Knopf.

Structure

Robertson, Roland. 1969. "Talcott Parsons." Pp. 214–22 in *The Founding Fathers of Social Science*, edited by Timothy Raison. Baltimore: Penguin.

Rocher, Guy. 1975. *Talcott Parsons and American Sociology*, translated by Stephen and Barbara Mennell. New York: Barnes & Noble.

Rosaldo, Renato. (1983) 1984. "Grief and the Headhunter's Rage." Pp. 178–95 in *Text, Play and Story*, edited by Edward Bruner. Washington, D.C.: AES Proceedings.

Ross, Dorothy. 1979. "The Development of the Social Sciences." Pp. 107–38 in *The Organization of Knowledge in Modern America, 1860–1920*, edited by Alexandra Oleson and John Voss. Baltimore: Johns Hopkins University Press.

Rossi, Ino. 1981. "Transformational Structuralism: Lévi-Strauss's Definition of Social Structure." Pp. 51–80 in *Continuities in Structural Inquiry*, edited by Peter M. Blau and Robert K. Merton. Beverly Hills, Calif.: Sage.

Samuelson, Franz. 1981. "The Struggle for Scientific Authority: The Reception of Watson's Behaviorism, 1913–1920." *Journal of the History of the Behavioral Sciences* 17:399–425.

Savage, Stephen P. 1981. *The Theories of Talcott Parsons*. New York: St. Martin's.

Schutz, Alfred. (1932) 1967. *The Phenomenology of the Social World*, translated by George Walsh and Frederick Lehnert. Evanston, Ill.: Northwestern University Press.

—. (1940–41) 1978. Letters to Talcott Parsons. Pp. 4–60, 71, 94–106, and 110–11 in *In The Theory of Social Action*, edited by Richard Grathoff. Bloomington: Indiana University Press.

Schwanenberg, Enno. 1971. "The Two Problems of Order in Parsons' Theory: An Analysis from Within." *Social Forces* 47:567–81.

—. 1976. "On the Meaning of the General Theory of Action." Pp. 35–45 in *Explorations in General Theory in Social Science*, vol. 1. Edited by Jan J. Loubser, Rainer C. Baum, Andrew Effrat, and Victor Meyer Lidz. New York: Free Press.

Sciulli, David. 1984. "Talcott Parsons's Analytical Critique of Marxism's Concept of Alienation." *American Journal of Sociology* 90:514–40.

—. 1986. "Voluntaristic Action." *American Sociological Review* 51:743–66.

Sciulli, David, and Dean Gerstein. 1985. "Social Theory and Talcott Parsons in the 1980s." *Annual Review of Sociology* 11:369–87.

Scott, John Finley. 1962. "The Impossible Theory of Action: Some Questions on Parsons' Prewar Classification of Action Theories." *Berkeley Journal of Sociology* 7:51–62.

—. 1963. "The Changing Foundations of the Parsonian Action Scheme." *American Sociological Review* 28:716–35.

—. 1971. *The Internalization of Norms*. Englewood Cliffs, N.J.: Prentice-Hall.

—. 1974. "Interpreting Parsons' Work: A Problem in Method." *Sociological Inquiry* 44:58–61.

Seidman, Steven. 1983. *Liberalism and the Origins of European Social Theory*. Berkeley: University of California Press.

Sewell, William H., Jr. 1987. "Theory of Action, Dialectic, and History." *American Journal of Sociology* 43:166–72.

Shils, Edward. (1961) 1965. "The Calling of Sociology." Pp. 1405–47 in *Theories of Society*, edited by Talcott Parsons, Edward Shils, Kaspar D. Naegele, and Jesse R. Pitts. New York: Free Press.

—. 1963. "The Contemplation of American Society." Pp. 392–410 in *Paths of American Thought*, edited by A. M. Schlesinger, Jr., and Morton White. Boston: Houghton Mifflin.

—. 1970. "Tradition, Ecology, and Institution in the History of Sociology." *Daedalus* 99:760–825.

American Journal of Sociology

Simpson, George. 1938. "Individual and Social Action." *New Republic*, September 28, p. 223.

Smith, Dorothy E. 1979. "A Sociology for Women." Pp. 135–87 in *The Prism of Sex: Essays in the Sociology of Knowledge*, edited by Julia A. Sherman and Evelyn Torton Beck. Madison: University of Wisconsin Press.

Somit, Albert, and Joseph Tanenhaus. 1967. *The Development of American Political Science*. New York: Irvington.

Sorokin, Pitirim. 1947. *Society, Culture, and Personality*. New York: Harper.

_____. 1963. *A Long Journey*. New Haven: College and University Press.

_____. 1966. *Sociological Theories of Today*. New York: Harper & Row.

Swidler, Ann. 1986. "Culture in Action: Symbols and Strategies." *American Sociological Review* 51:273–86.

Taussig, F. W. 1924. "Alfred Marshall." *Quarterly Journal of Economics* 39:1–14.

Taylor, John G. 1979. *From Modernization to Modes of Production*. London: Macmillan.

Taylor, O. H. (1936) 1967. "Economic Theory and Certain Non-economic Elements in Social Life." Pp. 380–90 in *Explorations in Economics: Notes and Essays in Honor of F. W. Taussig*. New York: Kelley.

Therborn, Göran. 1976. *Science, Class and Society*. London: New Left.

Tiryakian, Edward A. 1980. "Post-Parsonian Sociology." *Humboldt Journal of Social Relations* 7:17–32.

Tolman, Edward C. (1932) 1961. "A Summary Discussion of Purposive Behavior." Pp. 777–79 in *Theories of Society*, edited by Talcott Parsons, Edward Shils, Kaspar D. Naegele, and Jesse R. Pitts. New York: Free Press.

Traugott, Mark. 1978. "Introduction." Pp. 1–39 in *Emile Durkheim on Institutional Analysis*, by Emile Durkheim. Chicago: University of Chicago Press.

Turner, Jonathan M. 1987. "Parsons's *The Structure of Social Action*." Paper presented at the American Sociological Association meetings, Chicago.

Turner, Jonathan M., and Leonard Beeghley. 1974. "Current Folklore in the Criticisms of Parsonian Action Theory." *Sociological Inquiry* 44:47–55.

Turner, Stephen P. 1986. "The Emergence of Quantitative Sociology." Unpublished manuscript. University of South Florida, Department of Sociology.

Veblen, Thorstein. (1898) 1919. "Why Is Economics Not an Evolutionary Science?" Pp. 56–81 in *The Place of Science in Modern Civilisation*, by Thorstein Veblen. New York: Viking.

_____. (1899–1900) 1919. "The Preconceptions of Economic Science." Pp. 82–179 in *The Place of Science in Modern Civilisation*, by Thorstein Veblen. New York: Viking.

Vidich, Arthur J., and Stanford M. Lyman. 1985. *American Sociology: Worldly Rejections of Religion and Their Directions*. New Haven: Yale University Press.

Wagner, Helmut R. 1979. "Theory of Action and Sociology of the Life-World." *Contemporary Sociology* 8:685–87.

Waldo, Dwight. 1975. "Political Science: Tradition, Discipline, Profession, Science, Enterprise." Pp. 1–104 in *The Handbook of Political Science*, vol. 1. Edited by Fred I. Greenstein and Nelson Polsby. Reading, Mass.: Addison-Wesley.

Warner, R. Stephen. 1978. "Toward a Redefinition of Action Theory: Paying the Cognitive Element Its Due." *American Journal of Sociology* 83:1317–49.

Wearne, Bruce C. 1981. "Talcott Parsons's Appraisal and Critique of Alfred Marshall." *Social Research* 48:816–51.

_____. 1982. "Parsons's Sociological Ethic and the Spirit of American Internationalism: A Bibliographic Essay." Paper presented at the SAANZ conference.

_____. 1983. "Parsons's Theory as a Secularized Post-Calvinist Humanism: A Bibliographical Essay." Paper presented at the SAANZ conference.

Structure

Whalen, Jack. 1987. "Sociology as a Natural Observational Science." *Contemporary Sociology* 16:753–56.

Whitaker, J. K. (1977) 1982. "Some Neglected Aspects of Alfred Marshall's Economic and Social Thought." Pp. 453–86 in *Alfred Marshall: Critical Assessments*, vol. 1. Edited by John Wood Cunningham. London: Croom Helm.

Whitehead, Alfred North. (1925) 1967. *Science and the Modern World*. New York: Free Press.

Wiebe, Robert H. 1967. *The Search for Order*. New York: Hill & Wang.

Williams, Raymond. 1977. *Marxism and Literature*. New York: Oxford University Press.

Williams, Robin M., Jr. (1961) 1976. "The Sociological Theory of Talcott Parsons." Pp. 64–99 in *The Social Theories of Talcott Parsons*, edited by Max Black. Carbon-dale: Southern Illinois University Press.

_____. 1971. "Change and Stability in Values and Value Systems." Pp. 123–59 in *Stability and Social Change*, edited by Bernard Barber and Alex Inkeles. Boston: Little, Brown.

_____. 1980. "Talcott Parsons: The Stereotypes and the Realities." *American Sociologist* 15:64–66.

Wilson, John. 1983. *Social Theory*. Englewood Cliffs, N.J.: Prentice-Hall.

Wilson, R. Jackson. 1968. *In Quest of Community*. London: Oxford University Press.

Wirth, Louis. 1939. "Review of *The Structure of Social Action*." *American Sociological Review* 4:399–404.

Wolff, Kurt H., ed. 1980. *Human Studies* (Special Issue in Memory of Talcott Parsons) 4:309–402.

Wood, John Cunningham. 1982. "Introduction and General Commentary." Pp. xiv–xxxiii in *Alfred Marshall: Critical Assessments*, vol. 1. Edited by John Wood Cunningham. London: Croom Helm.

Wrong, Dennis H. 1961. "The Oversocialized Conception of Man in Modern Sociology." *American Sociological Review* 26:183–93.

Young, Allyn A. 1927. "Economics as a Field of Research." *Quarterly Journal of Economics* 42:1–25.

Zaret, David. 1980. "From Weber to Parsons and Schutz: The Eclipse of History in Modern Social Theory." *American Journal of Sociology* 85:1180–1201.

Zimmerman, Don H., and D. Lawrence Wieder. 1970. "Ethnomethodology and the Problem of Order." Pp. 285–98 in *Understanding Everyday Life*, edited by Jack D. Douglas. Chicago: Aldine.

Znaniecki, Florian. 1934. *The Method of Sociology*. New York: Farrar & Rinehart.

_____. 1952. *Cultural Sciences*. Urbana: University of Illinois Press.

Fathers' Ages and the Social Stratification of Sons¹

Robert D. Mare and Meei-Shenn Tzeng
University of Wisconsin—Madison

The reproduction of inequality from generation to generation depends not only on the degree to which parents transmit their statuses to their offspring but also on the ages at which parents bear their offspring. Because parents' education, employment, and economic wealth improve with age, and because younger parents experience stronger competing role demands than older parents, parental age influences children's environments. The average number of siblings also varies with the age of the parents, which affects the family resources to which a child has access. As a result, when other aspects of son's socioeconomic background are controlled, sons born to older fathers enjoy significantly higher levels of educational and occupational achievement than sons born to younger fathers.

Inequalities persist across generations through the reproduction of individuals and the transmission of social positions and statuses from parents to offspring. Parents transmit their social standing to their children through their varying capacities to provide favorable family and educational environments. Parents also affect the success of their offspring through the number of their children and the *timing* of fertility within parents' lives. A neglected aspect of the process by which parents affect their offspring is the link between the timing of fertility in the parents' generation and the socioeconomic achievement of offspring. This article

¹ An earlier version of this article was presented at the 1988 meetings of the Population Association of America. This research was supported by the National Institute of Aging (PO1 AGO4877) and the Graduate School of the University of Wisconsin—Madison. Computations were performed using facilities of the Center for Demography and Ecology at the University of Wisconsin—Madison, which are supported by the Center for Population Research of the National Institute for Child Health and Human Development (HD-5876). We are grateful to Robert Hauser, Peter Mueser, Judith Seltzer, Herbert Smith, and anonymous reviewers for helpful comments on an earlier draft and to Christine Winquist Nord for her suggestions at an early stage of this research. Requests for reprints should be sent to Robert D. Mare, Department of Sociology, University of Wisconsin—Madison, Wisconsin 53706.

Fathers and Sons

reports an investigation of the effects of fathers' ages at the births of their sons on their sons' educational and occupational achievements.

Studies of socioeconomic achievement typically measure the family backgrounds of individuals by the socioeconomic levels of their parents, as well as by the size and structure of the families that they grow up in (see, e.g., Featherman and Hauser 1978; Hauser, Tsai, and Sewell 1983). Such studies usually ignore that the socioeconomic environments provided by parents depend on their life-cycle positions. Individuals and families follow typical cycles from the stages of marriage and childbearing through the "empty nest" stage, but they vary in how they fit these events and statuses into their lives (Elder 1978; Taeuber and Sweet 1976; Oppenheimer 1982). Parents vary in the ages at which they bear children and in their other statuses and activities before, during, and after child rearing. If the statuses and activities of parents constrain and benefit children, the way that parents organize their lives in time may affect their children's environments and thus constitute an influential aspect of children's family backgrounds.²

Few studies of socioeconomic achievement take account of the changing environments that children may experience as a result of parental aging, though some emphasize the effects of the timing of events on achievement and emphasize the dynamic aspects of family background. Hogan (1981) examines the consequences for men's socioeconomic achievement of the timing and ordering of events when they are young adults. Alwin and Thornton (1984) study the effects on educational attainment of offspring of their family background conditions at several points in their childhoods. Featherman and Spenner (1988) describe changes in class backgrounds of children during their first 18 years. We examine the effects of parental age on the achievements of children.

THE EFFECTS OF PARENTAL AGE ON THE ACHIEVEMENT OF OFFSPRING

"Early" and "Delayed" Childbearing

Research on the effects of parents' ages on the welfare of their offspring emphasizes the special circumstances experienced by children born to teenage mothers (see, e.g., Hayes 1987; Hofferth 1987*a*, 1987*b*). Children born to teenage women suffer a number of handicaps. Compared with the children born to women in their twenties, children of teenagers score slightly lower on measures of IQ, intellectual achievement, and socioemo-

² Parents' ages at childbearing may also affect their children's socioeconomic welfare later in life by determining the ages at which children can expect to look after their aged parents and inherit parental wealth.

tional development. Such effects are mainly attributable to the lower socioeconomic levels of families in which women bear children early, although they may also be a result of the relatively poor quality of prenatal care obtained by teenage mothers. The effects, moreover, are typically small relative to other aspects of family structure and socioeconomic status. (Hofferth [1987b] provides a comprehensive review of this research.) Research on the effects of maternal age on children's well-being usually compares teenage mothers with those only somewhat older and does not systematically investigate parental age effects. Such research, moreover, usually focuses on mothers to the exclusion of fathers and on children who are firstborn rather than on all children. Nor has it examined the long-range effects of parental age on the welfare of offspring when they have reached adulthood. While informative, therefore, this work does not resolve the more general issues of whether parental age effects can be detected among children not born to teenagers and whether parental age affects the socioeconomic achievement of offspring when they become adults.

To a limited degree, researchers also consider the social and economic circumstances of families in which parents delay childbearing past their twenties (e.g., Baldwin and Nord 1984; Hofferth 1984). Women who bear children later are better off economically throughout the balance of their lives than women who bear children earlier, a benefit that presumably accrues to their offspring as well (Hofferth 1984). Children of older parents may also benefit from their parents' greater emotional maturity compared with younger parents', but older parents may have less flexibility and energy to face the rigors of child rearing than their younger counterparts. The possible advantages and disadvantages for parent-child relations that derive from older parenting are the subject of considerable speculation but little systematic research (Baldwin and Nord 1984; Gerson 1985).

Sibship Size, Birth Order, and Age at Onset of Childbearing

Children born to parents at varying ages may differ markedly both in the number of siblings with whom they share family resources and in their parents' overall childbearing patterns (Oppenheimer 1982). Children born to older parents are typically higher-order births and have, on average, more siblings than children born to younger parents. To a lesser degree, children born to teenage parents also experience large sibships because their parents are at the risk of childbearing for such a long period of time. Because large sibship size handicaps achievement (see, e.g., Hauser and Sewell 1985; Mare and Chen 1986), children born late in parents' childbearing years and, to some degree, those born to teenage

parents may enjoy less socioeconomic success than children born to parents at other ages. It is unlikely, however, that differences in socioeconomic achievement among children born to parents of different ages result from differences in birth order per se. Detailed analyses of birth-order effects on educational attainment show that apparent birth-order differences are entirely the result of differences in sibship size, birth cohort, and parents' socioeconomic characteristics (Hauser and Sewell 1985).

Among children who differ in their parents' ages at birth but have the same number of siblings, however, children born to older parents may have a distinct advantage. Those born to younger parents are more likely to enter a family where parents initiated childbearing at an earlier age. Given the large financial outlay associated with the first birth (Espenshade 1984) and the rising socioeconomic standing of adults with age, children born to parents who began their childbearing early may be disadvantaged relative to those whose parents started childbearing when they were more financially secure. By this argument, the effects of parents' ages on a child may reflect differences in the parents' ages when the *first* child in their family was born rather than in the exact ages of the parents when each child in a family was born.³

Age Patterns of Socioeconomic Status and Their Effects on Offspring

Parents' ages at children's births may affect children's socioeconomic success through differences in socioeconomic environments that parents can provide. Persons typically improve their socioeconomic positions from their teens to middle adulthood and then experience some loss of socioeconomic position with retirement. Their average educational attainment rises throughout the childhood and teenage years and peaks in the late twenties. Their earnings, incomes, and wealth typically rise, albeit at a decreasing rate, from the teenage years into their fifties (see, e.g., Becker 1975; Hauser, forthcoming; Henretta and Campbell 1978). They usually enjoy upward occupational mobility from their initial jobs in their teens or twenties to the jobs that they hold in their "prime" adult years (see, e.g., Featherman and Hauser 1978). Their chances of avoiding unemployment also rise with age, sharply during the teenage years and then more gradually through their twenties and thirties (see, e.g., Mare, Win-

³ The relationship between parental age and offspring's socioeconomic success is further complicated because children born to parents in the *middle* of their childbearing years may enter families that have relatively many children *present in the household* compared with children born to younger or older parents. Such children may be handicapped because their early childhood coincides with the time during which their parents have their maximum number of dependents (Oppenheimer 1982).

American Journal of Sociology

ship, and Kubitschek 1984). Inasmuch as parents' education, occupational standing, earnings, and employment typically increase with their ages, the socioeconomic achievements of offspring born to relatively older parents may exceed those of children born to relatively younger ones.

Because most parents' socioeconomic statuses peak during the prime ages of adulthood or increase only modestly during middle age, the rate of increase in benefits that children enjoy with rising parental ages may decline as parents age. Children born to the very oldest parents, moreover, may suffer a drop in family living standards if parents' retirement coincides with the financing of the children's higher education. It is, however, unclear whether the weakened financial position of aging parents affects their offspring's attainments. The strength of the effect depends on the relative importance of the effects of parents' socioeconomic status at retirement and of their stream of statuses throughout their children's lives.

Role Demands on Young and Old Parents

Beyond the differential advantages that children may derive from variation in parents' socioeconomic status with age are additional potential effects of parents' ages at children's births. Adults of varying ages differ in their roles and statuses, and that may affect the way they allocate their time and money between children and other demands. Young adults are more likely than older adults to experience a multiplicity of role demands that may affect the time they can devote to children. One or both young parents may still be in school and thus face constraints on the time and money that they can devote. Persons in their twenties may also be more likely than older persons to be at a point in their work careers when extra time investments are required to ensure job stability or advancement. In addition, whether young adults have become parents or not, they are more involved than older adults in social and recreational activities (Hill 1985), which place further demands on their time and money. Taken together, these age patterns of role demand and time use suggest that resources available to parents for their children may increase with age, even within groups that are relatively homogeneous in socioeconomic standing.

Some support for this conjecture is provided by research on parents' allocation of both time and money to children. Time-use studies suggest that parents' average time spent with children rises with parents' ages from the teenage years onward, reaching a peak for 45–54-year-old men and 35–44-year-old women (Juster 1985, pp. 194–96). This result holds among parents with similar family income, ethnicity, educational attainment, home ownership, labor-force participation, marital status, child-

Fathers and Sons

bearing status, and children's ages. With regard to the association between expenditures on children and ages of parents, evidence also shows an upward gradient with parents' ages. Espenshade (1984, p. 62) shows that when age of child, mother's labor-force participation, and socioeconomic status are controlled, average expenditures on children rise monotonically with age of mother at her first birth. These results indicate that even within groups that are relatively homogeneous in socioeconomic standing, older parents do invest more time and money in their children. Such patterns may result in rising socioeconomic achievement of offspring as parents age, net of the effects of the well-known family socioeconomic background conditions.

DATA AND METHODS

Our analyses are directly applied to some of the ideas discussed above and indirectly provide evidence on others. We describe the associations among parents' ages, other family background factors, and the achievements of offspring. We regard father's age at son's birth as an exogenous variable that is one among many correlated aspects of son's background that affect his educational and occupational achievement. We rely on survey data that contain information on ages of fathers when sons were born, the socioeconomic characteristics of sons' families when they were children and teenagers, sons' numbers of siblings, and sons' educational and occupational achievements. Thus, we interpret the gross effects of fathers' ages on sons' achievements and the net effects that remain after family socioeconomic and demographic characteristics are taken into account.

Measurement and Sample Definition

Our analyses use the 1973 Occupational Changes in a Generation survey (OCG), which represents men aged 20–65 in the civilian noninstitutional population of the United States (Featherman and Hauser 1975). The OCG data permit the kinds of analyses reported here because, in addition to standard measures of family background and socioeconomic achievement, they include a measure of father's age at son's birth. This measure is derived from responses to the question "In what year was your father born?"

The main dependent variables in our analysis are several measures of respondent's educational attainment, including his highest grade of school completed and a sequence of dichotomous variables that denote whether he made selected school transitions. These transitions include (1) completes elementary school (completes eighth grade); (2) attends high

school, given completes elementary school (attends ninth, given completes eighth); (3) completes high school, given attends high school (completes twelfth, given attends ninth); (4) attends college, given graduates from high school (attends thirteenth, given completes twelfth); (5) completes college, given attends college (completes sixteenth, given attends thirteenth); and (6) attends postcollege schooling, given graduates from college (attends seventeenth, given completes sixteenth). By partitioning schooling into transitions, we can investigate variation in the effects of father's age by stages of the schooling process, which may depend differentially on family resources and circumstances (Kerckhoff 1974; Mare 1980). We also report the effects of father's age on son's occupational attainment, including the occupational statuses of his first full-time job after completing school and his job in March 1973. Occupational status is measured by means of the Duncan (1961) socioeconomic index (SEI).

In addition to father's age at son's birth, our analyses include: status of father's occupation when respondent was age 16, as measured by the Duncan SEI; father's and mother's highest grades of school completed; family income when respondent was age 16, measured in thousands of constant (1967) dollars; number of siblings; whether the respondent lived on a farm at age 16; whether the respondent was born in the southern region of the United States; whether the respondent is black; and respondent's birth cohort, as measured by dichotomous variables that denote membership in one of nine five-year cohorts (1907–11, 1912–16, . . . , 1947–51).

A limitation of this study is that our data provide information on the age of fathers when sons were born, but not on parents' ages when each child in the family was born. Thus, we do not observe when parents began childbearing, and cannot distinguish between the effects of parents' ages when a given child was born and the effects of parents' ages when their *first* child was born.⁴ We also lack data on age of mother at births. Because assortative mating based on ages of potential spouses is strong, however, the relationships between father's age and son's achievements may be similar to those that would be observed between mother's age and offspring's achievement.

Of the 33,613 OCG respondents, our analysis of son's highest grade of school completed is based on 24,602 who (1) had complete data on son's

⁴ It is possible to identify respondents' birth order in our data, but this is an inadequate basis for assessing birth-order effects because we do not know the order, parents' ages at birth, and achievements of respondents' siblings. In analyses not reported here, we examined the effects of father's age on son's schooling with respondent's birth order controlled. The resulting estimates of father's age effects are very similar to those reported here.

Fathers and Sons

and father's year of birth, (2) were aged 21–65 in 1973, (3) reported that their fathers were between ages 15 and 70 when they were born, and (4) were living with their fathers most of the time up to the age of 16.⁵ We excluded 20-year-olds because many of them were still in school in 1973. Sons reporting that, at their births, their fathers were younger than 15 or older than 70 were excluded because their reports are likely to be in error. Although respondents reported the year of birth of family heads if they did not live with their fathers, we excluded such persons because they typically reported their mothers' ages at birth. To include such persons would confound the influences of being raised in a single-parent household with the differences in effects of maternal and paternal ages. As a result of this decision, our subsample somewhat underrepresents blacks and persons from lower socioeconomic groups. The analyses of son's first and 1973 occupations are based on slightly smaller sample sizes because of missing data on these two dependent variables. In the analyses of school transitions, we subsampled men eligible for each of the first five transitions in order to speed computation and to obtain approximately equal statistical power for each transition. Because the number of men eligible to make school transitions declines as men go further in school, we subsampled at a rate of 25% for the first three transitions, 40% for the fourth and fifth transitions, and 100% for the final transition. The resulting sample sizes vary between 4,500 and 7,000.

Statistical Methods

To investigate the effects of father's age at son's birth on son's socioeconomic achievement, we use linear and logistic regression models that provide estimates of the net effects of father's age, family background, and son's birth cohort.⁶ Using the estimated coefficients from these models, we compute the means of the dependent variables for sons with fathers of varying ages, adjusted for differences in family background and cohort membership of sons. The adjusted means are the levels of educational attainment that sons born to fathers in each age group would achieve if their cohort and family background composition were that of the sample as a whole.

⁵ Of the 33,613 respondents, 5,072 were not living with their fathers most of the time up until age 16, 857 were age 20 in 1973, 2,751 were missing data on father's year of birth or son's age, and 331 reported a father's year of birth that implied an unreasonable age at son's birth (i.e., less than 15 or greater than 70).

⁶ In this paper we present no results on possible interactions between father's age and social background factors. Analyses not reported here indicate that the effects of socioeconomic background and father's age are essentially additive (Tzeng 1987).

RESULTS

Father's Age, Social Background, and Achievement

Table 1 summarizes the average values of socioeconomic background, school, and occupational characteristics of men classified by the ages of their fathers when they were born. All means are adjusted for son's birth cohort to take account of potential associations between father's age at son's birth, son's year of birth, and secular trends in social characteristics.⁷ Educational attainment among sons does indeed vary with ages of fathers. Average number of years of school completed varies curvilinearly, rising from 10.8 years for sons of teenage fathers to 12.2 years for sons born to men in their early thirties and falling again to 10.8 years for sons of middle-aged fathers. Table 1 also shows that the status of son's first and 1973 occupations also varies curvilinearly with father's age. The SEI scores for both occupations peak for sons born to 30–34-year-old men, at approximately eight points above the scores for sons born to either the youngest or oldest men. These differences can be illustrated with specific occupations. For example, 1970 census occupational titles with SEI scores ranging from 41 to 44 include "electricians," "utility meter readers," and "statistical clerks"; scores 33–36 include "sheriffs and bailiffs," "sheet metal apprentices," and "plumbers and pipe fitters"; and scores 25–28 include "plasterers," "household appliance installers and mechanics," and "ushers, recreation and amusement" (Featherman, Sobel, and Dickens 1975). Differences this large in occupational standing are approximately the same as those produced by differences of two years of son's schooling (see below). Both son's educational and his occupational attainments, therefore, vary substantially with father's age.

We also examine patterns of family background factors across fathers' age groups. In general, socioeconomic characteristics of sons' families of orientation vary curvilinearly with father's age at son's birth. Family socioeconomic characteristics are most favorable to sons born to 30–34-year-old men and least favorable to sons born to fathers who are aged 50 and above. Sons born to the youngest fathers are also disadvantaged, though not so much as those born to middle-aged fathers. For example, average family incomes for the 16-year-old sons' families rise from \$6,900 for the sons of teenage fathers to \$8,600 for those with 30–34-year-old fathers and fall to \$5,300 for those with middle-aged fathers. These income patterns, of course, measure family circumstances when the sons were aged 16, that is, when the 15–19-year-old fathers were 31–35, when

⁷ We adjust characteristics for son's birth cohort by regressing them on dummy variables for the eight five-year categories of father's age and the nine five-year son's birth cohorts. That is, we compute means for each category of father's age for a hypothetical group having the same son's birth cohort composition as the total sample.

TABLE 1
SELECTED CHARACTERISTICS OF SONS BY FATHER'S AGE AT SON'S BIRTH

CHARACTERISTIC	FATHER'S AGE AT SON'S BIRTH				
	15-19	20-24	25-29	30-34	35-39
Social background:					
Father's schooling	8.2	8.7	8.9	8.8	8.2
Mother's schooling	7.9	8.8	9.1	9.1	8.4
Father's SEI	25.7	28.8	30.6	32.1	29.9
Percentage farm background	2.1	2.2	2.1	2.2	2.6
Family income (\$1,000s)	6.9	7.8	8.4	8.6	8.1
Number of siblings	2.5	2.2	2.4	2.8	3.5
Percentage black	20.9	12.7	9.7	8.2	8.3
Percentage born in South	42.1	37.9	32.2	29.6	30.2
Socioeconomic attainment:					
Years of schooling	10.8	11.7	12.0	12.2	12.1
SEI of first occupation	25.0	30.8	33.5	34.7	33.7
SEI of 1973 occupation	34.5	39.7	41.3	42.2	41.0
Percentage of observations:					
Total	2.2	14.9	26.5	24.4	16.3
White	1.9	14.4	26.6	24.8	16.6
Black	4.7	18.9	25.9	20.2	13.6
N	543	3,657	6,522	5,996	4,010
				2,175	956
					743

NOTE.—All means are adjusted for son's age in 1973 (birth cohort).

American Journal of Sociology

the 30–34-year-old fathers were 46–50, and when the fathers older than 50 were aged 66 and above. Thus, teenage sons whose fathers were teenagers when they were born benefit from higher family incomes than sons born to middle-aged fathers because many of the latter fathers are retired when their sons are aged 16.

Father's occupational status and mother's and father's schooling vary across fathers' age groups in a pattern and degree that are similar to the variation in educational and occupational attainments of sons themselves. The racial composition of father's age group also varies curvilinearly with father's age. Black sons are born disproportionately to teenage and, to a somewhat lesser extent, middle-aged fathers. Conversely, blacks are underrepresented among sons born to men in their thirties. This pattern is mirrored in the proportions of sons born in the South for different fathers' age groups, mainly a result of the overrepresentation of blacks in that region. Unlike most of the family background measures, son's number of siblings varies directly with father's age at son's birth, rising from 2.5 for sons born to teenage fathers to 5.7 for sons born to men aged 50 and older. This pattern no doubt arises because men achieve high levels of fertility in part by having children over a broad span of ages. Finally, we observe a similar age pattern of son's farm background composition, which reflects both relatively high farm fertility and that, within sons' cohorts, persons with farm backgrounds are disproportionately the sons of earlier cohorts of fathers because of secular declines in farm occupations. In sum, differences in the social and economic backgrounds of sons born to men of varying ages are large and may account for at least some of the association between educational and occupational achievements of sons and the ages of their fathers.

Net Effects of Father's Age on Son's Educational Achievement

Son's educational attainment varies with father's age, but, as we have shown, many aspects of sons' families of orientation also vary systematically with parental age. Age patterns of fertility vary with parents' socioeconomic characteristics, and socioeconomic welfare itself varies with parents' ages. The apparent effects of father's age at son's birth on son's educational attainment may arise because of the associations between father's age and family socioeconomic background. To investigate this, we examine regression models that include effects of both social background and father's age on son's schooling (see table 2). All models include the effects of son's birth cohort, although we do not report these.⁸

⁸ In all models, the net effects of cohort show secularly increasing levels of son's schooling, consistent with previous analyses of these data (Hauser and Featherman 1976; Mare 1979).

Fathers and Sons

TABLE 2

EFFECTS OF FATHER'S AGE AND FAMILY BACKGROUND ON SON'S HIGHEST GRADE OF SCHOOL COMPLETED

VARIABLE	SELECTED MODELS					
	β	t	β	t	β	t
Constant	9.073	59.2	5.813	4.3	4.930	3.6
Father's age at son's birth:						
15–19
20–24883	6.0			.451	3.8
25–29	1.248	8.8			.663	5.7
30–34	1.448	10.2			.843	7.3
35–39	1.287	8.8			1.060	9.0
40–44	1.047	6.8			1.200	9.6
45–49791	4.6			1.222	8.7
50+040	.2			1.191	8.0
Father's schooling105	16.4	.113	17.6
Mother's schooling191	28.7	.191	28.9
Family income069	20.9	.069	20.9
Father's SEI016	15.8	.015	14.8
Farm			−.758	−16.6	−.806	−17.7
Number of siblings			−.155	−25.4	−.168	−27.3
Black			−.284	−4.6	−.224	−3.7
South			−.333	−8.4	−.298	−7.6
R^2070		.384		.390	

NOTE.—Analyses are based on 24,602 observations. All models include dichotomous variables that denote son's five-year birth cohort (1912–16, . . . , 1947–51) and whether data are present on each of the social background variables except race and farm background, which have no missing data; t = ratio of coefficient to its standard error.

The first two columns of table 2 present contrasts among fathers' age groups in son's schooling. These contrasts, which are the basis for the adjusted means of son's schooling reported in line 2 of figure 1, show the curvilinear pattern of son's educational attainment with father's age. They account for only a small proportion of the variance in son's schooling, but they are large and statistically significant. Figure 1 also shows that the pattern of son's educational attainment by father's age is largely unaffected by the control for son's cohort.

The last two columns of table 2 present the effects of both father's age and social background characteristics of sons. The effects of social background on son's educational attainment are well established and will not be discussed in detail here (see Featherman and Hauser 1978; Hauser and Featherman 1976; Mare 1980).⁹ The net effects of father's age differ

⁹ The effect of race on son's educational attainment in table 2 is smaller than shown in previous analyses of these data (Hauser and Featherman 1976, p. 105). This difference

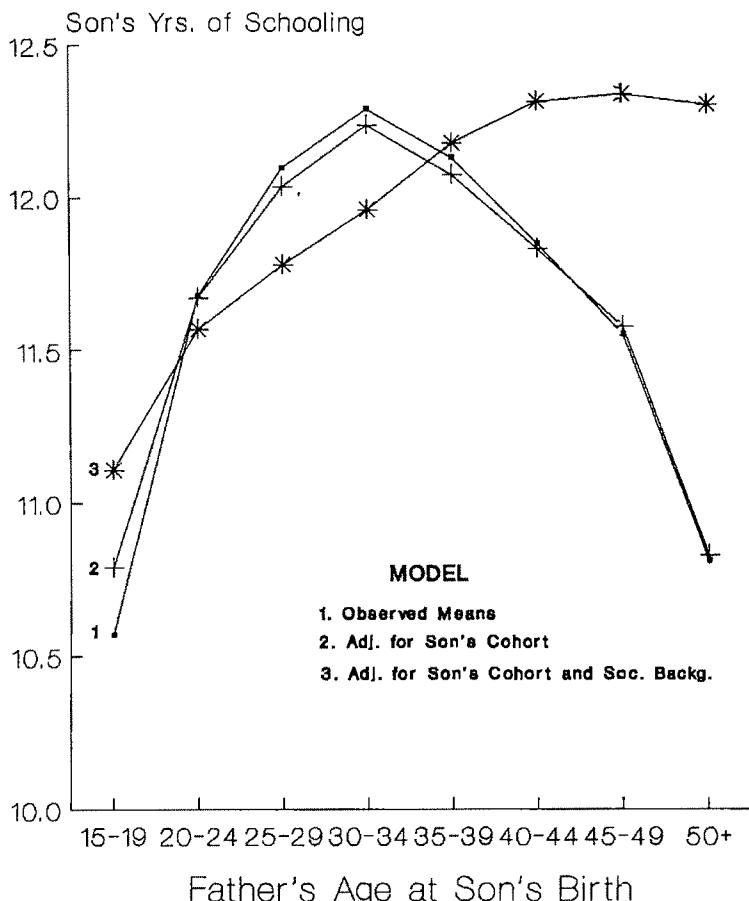


FIG. 1.—Observed and adjusted means of son's schooling by father's age at son's birth.

substantially from those that are not adjusted for social background factors. In particular, father's older age affects son's highest grade of schooling positively and, except for the oldest father's age group, monotonically.

arises because our models include the effects of mother's schooling and family income, whereas previous analyses have not. Race differences in these two variables account for a significant part of the race difference in schooling. The race coefficient is an average effect over a period when the black disadvantage in schooling declines (Featherman and Hauser 1978). In analyses not reported here, we estimated models that include the interaction between race and son's birth cohort, which show a secularly declining race effect. The estimated effects of father's age and the other variables in these models, however, are the same as those presented in table 2. For simplicity and to save space, we present results only for models that include additive race effects.

cally. Line 3 in figure 1 shows that son's schooling rises steeply as father's age at son's birth increases from the teens to the twenties, and more gradually thereafter. Adjusted levels of son's schooling vary from about 11.1 years of schooling for men with teenage fathers to 12.3 years for sons of middle-aged fathers. One can assess the importance of this difference of 1.2 years by comparing it with the effects of other social background factors in the model. For example, this difference is greater than that between men whose fathers' schooling differed by more than 10 years [$1.2 > (10 \times 0.113)$] and more than five times the difference between black and white men who are equivalent in other aspects of social background. In short, the effects of the timing of a son's birth within the father's lifetime are important to the son's educational achievement.¹⁰

To investigate the degree to which specific social background differences among men with fathers of varying ages account for the observed curvilinear pattern of son's schooling effects, we use the model of father's age and social background effects to decompose differences in son's schooling between selected pairs of ages. Table 3 shows the components of differences in average levels of schooling between sons with fathers aged 15–19 and 30–34 and between sons with fathers aged 30–34 and 50 and older.¹¹ The observed advantage in schooling to men born to 30–34-year-olds over men born to teenagers is attributable in part to the more favorable background conditions measured by variables included in the model. Approximately 35% of the difference in schooling is due to measured socioeconomic differences (father's and mother's schooling, father's occupation, and family income) between men with differing fathers' ages. The largest part of the difference, however, is unexplained. For the contrast between sons of fathers aged over 50 and of those aged 30–34, however, more than 80% of the difference is attributable to the less favorable socioeconomic conditions of sons born to middle-aged fathers. In addition, sons born to fathers aged 50 and older suffer the handicap of larger sibships than men born to younger fathers. Differences in sibship size account for 35% of the total. These calculations imply that, taken alone, differences in family environments between those provided by

¹⁰ In models not shown here, we examined the effects of the interaction between father's age and son's cohort on son's schooling. These analyses show no significant or systematic interactions, suggesting that the positive effect of father's age on son's schooling shown in table 2 is stable over cohorts.

¹¹ The total differences in son's schooling between pairs of fathers' age groups are computed from the means reported in table 1. The component for each independent variable is $\beta_j(X_{j2} - X_{j1})$, where β_j denotes the effect of the j th social background variable in the model reported in col. 5 of table 2, and X_{j2} and X_{j1} are the means of the j th social background variable (reported in table 1) for the older and the younger fathers' age groups, respectively. The decomposition assumes no interaction between father's age and the effects of social background.

TABLE 3

DECOMPOSITION OF DIFFERENCES BETWEEN FATHERS' AGE GROUPS
IN SON'S YEARS OF SCHOOL COMPLETED

COMPONENT	15-19 AND 30-34		30-34 AND 50+	
	Difference (Years)	Percentage	Difference (Years)	Percentage
Father's schooling068	4.96	-.305	22.37
Mother's schooling229	16.76	-.178	35.00
Family income117	8.58	-.228	16.69
Father's SEI096	7.02	-.147	10.78
Farm	-.001	-.06	-.018	1.30
Number of siblings	-.050	-3.68	-.487	35.71
Black028	2.08	-.015	1.08
South037	2.72	-.035	2.58
Residual843	61.62	.348	-25.51
Total	1.367	100.00	-.1365	100.00

NOTE.—Decompositions are based on coefficients reported in col. 5 of table 2 and means reported in table 1.

fathers aged 30-34 and those provided by fathers aged 50 and older would lead to an even greater educational disadvantage to sons of the oldest fathers than is observed. As indicated by the positive residual, unmeasured factors partially offset this disadvantage.

Although the effects of father's age on son's schooling are large and father's age is associated with social background, differences in fathers' ages among persons with varying socioeconomic backgrounds do not account for measured background effects. A comparison of the coefficients reported in column 5 of table 2 with those for a model that excludes the effects of father's age (see col. 3) shows that background effects are largely unchanged when adjusted for father's age. Only the effect of race is partly explained by age differences of fathers. The overrepresentation of blacks among the youngest and oldest fathers' age groups explains approximately 20% of their educational handicap.

In summary, sons' educational achievement varies sharply with the timing of their births in their fathers' lives. Sons of men in their early thirties enjoy the highest levels of achievement, but this advantage is largely attributable to their highly favorable backgrounds, especially in parents' socioeconomic characteristics and sibship size. Once family background is controlled, son's schooling varies directly with father's age at son's birth, yielding a difference of about 1.2 grades of schooling between sons with the youngest and oldest fathers. Although only a small proportion of the variance in son's educational attainment is explained by

Fathers and Sons

father's age group, the effect of father's age on son's years of school completed is large and equivalent to that produced by large differences in other well-known determinants of educational attainment.

Father's Age and Son's School-Continuation Decisions

The changing effects of father's age across levels of the schooling process provide further information on how the timing of the son's birth affects his achievement. Father's age may affect son's schooling positively because social and economic resources of families, which are not fully reflected in measured parental statuses, accumulate with age and thus provide a relatively better environment for offspring of older parents. Such resources may include wealth, which is poorly measured by family income, as well as elements of "cultural capital" that may not be measured by parents' years of schooling. If such factors are important, then father's age may more strongly affect school transitions in which family wealth is most important for educational success, that is, post-secondary-school transitions.

Figure 2 plots son's probabilities of making selected school transitions (conditional on his eligibility to make the transitions) by father's age, adjusted for social background differences among fathers' age groups. These adjusted probabilities for each transition are computed from a logistic model in which the independent variables are the same as for the model reported in the final column of table 2.¹² The evidence for stronger effects of father's age at the college level is mixed. The effects of father's age on the log odds that sons graduate from college, given that they attend (13–16), are indeed stronger than on the odds of any other transition. For this transition, the effects of father's age are statistically significant and imply that the probability of graduation from college, given attendance, ranges from approximately .3 for sons born to teenagers to .6 for sons born to middle-aged fathers (a difference of about 1.4 in the logit scale). For the transition from high school graduation to college (12–13), however, where one might also expect the environment provided by older parents to be advantageous, the differences among fathers' age groups, although generally positive, are not statistically significant. Next to the transition from college attendance to graduation, the largest effects of father's age occur for the transition from high school attendance to gradu-

¹² Coefficients and significance tests for these models are not reported here. They are available from us on request. Adjusted log odds of school continuation are computed using the estimated logit coefficients from the models and the sample means of the independent variables. For each father's age group, these odds are smoothed to follow a third-degree polynomial, converted to probabilities, and plotted on the logit scale in fig. 2.

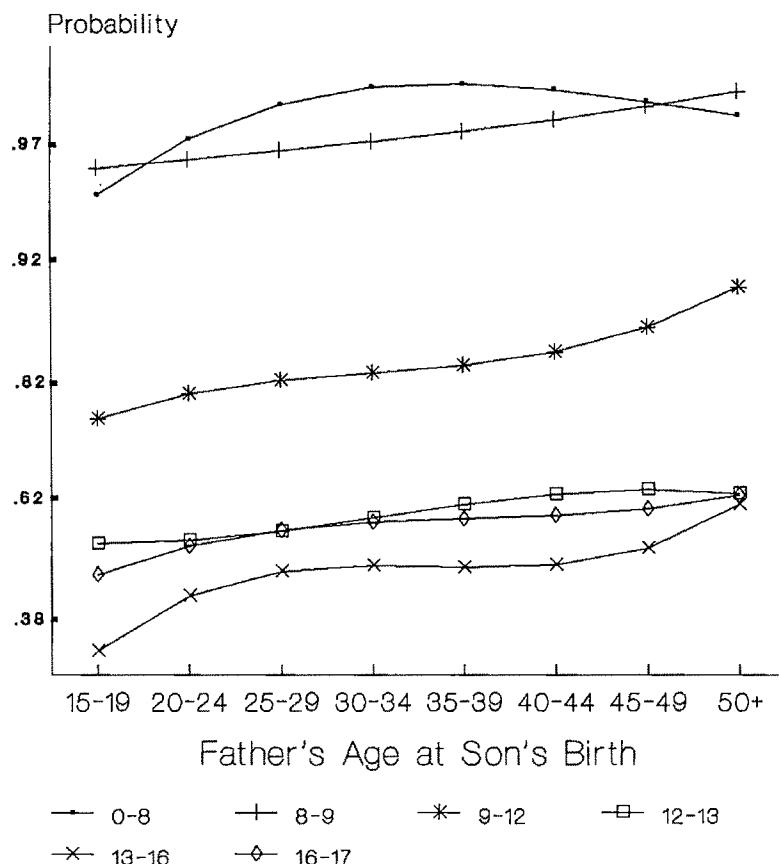


FIG. 2.—Adjusted school continuation probabilities by father's age at son's birth. Note that data are smoothed to follow a third-degree polynomial.

ation (9–12), for which adjusted probabilities rise with father's age from approximately .7 to slightly less than .9 (a difference of about 1.2 in the logit scale). The only other transition for which contrasts among fathers' age groups are significant is for completion of elementary school (0–8), but there is little evidence of variation among sons born to fathers who are not teenagers.

These results suggest that the more favorable environments provided by older fathers are particularly advantageous for sons' persistence in high school and college. This is broadly consistent with the argument that net parental resources that benefit offspring accumulate with age. That the effects of father's age on son's probabilities of college attendance are weak, however, suggests that these parental resources should not be interpreted solely as levels of economic wealth. The financial resources

Fathers and Sons

that are potentially available to college-age sons of older fathers, moreover, may also be needed by the fathers themselves as they approach retirement.

Net Effects of Father's Age on Son's Occupational Achievements

We next inquire whether father's age also affects son's occupational achievement. We examine the effects of father's age on the status of son's current (1973) and first occupations to investigate whether these effects persist when son's family background and schooling are taken into account. Table 4 reports the results of several regression models for the effects of selected combinations of father's age, family background, and son's schooling on son's current occupation. All models include the effects of son's birth cohort, which we do not report.¹³ Figure 3 graphs the adjusted means of son's occupational status that are implied by these models for both current and first occupations. The first two columns of table 4 report the effects of father's age at son's birth, with son's year of birth controlled for. The curvilinear pattern of coefficients, which shows a maximum advantage for sons born to 30–34-year-old men, is the same as that reported in table 1.

The second two columns of table 4 report that net effects of father's age after family background characteristics of sons are taken into account. The net coefficients for father's age and the corresponding adjusted means in lines 2 of figure 3 indicate that the net effects of father's age on son's occupation are generally positive. Once family background characteristics are taken into account, son's current occupational status rises monotonically with father's age from approximately 37 points for sons born to teenage fathers to approximately 43 points for sons of middle-aged fathers. Father's age at son's birth affects the status of son's first occupation somewhat more strongly, although there is some departure from monotonicity. Once family background characteristics are controlled, sons of the oldest group of fathers enjoy an occupational status that averages more than eight points higher than that of sons of teenagers.

These patterns are consistent with the positive effects of father's age on son's educational attainment and suggest that some of the economic and cultural advantages provided by older fathers that raise sons' educational attainment benefit their occupational standing as well. Alternatively,

¹³ Coefficients for models for son's first occupation are not reported here. They are available from us on request. In addition to the models discussed in the paper, we also estimated models that allowed for the separate effects of son's years of precollege and college schooling completed on his occupational statuses (Featherman and Hauser 1978). The estimated effects of father's age at son's birth from this specification are identical with those reported in table 4 and fig. 3.

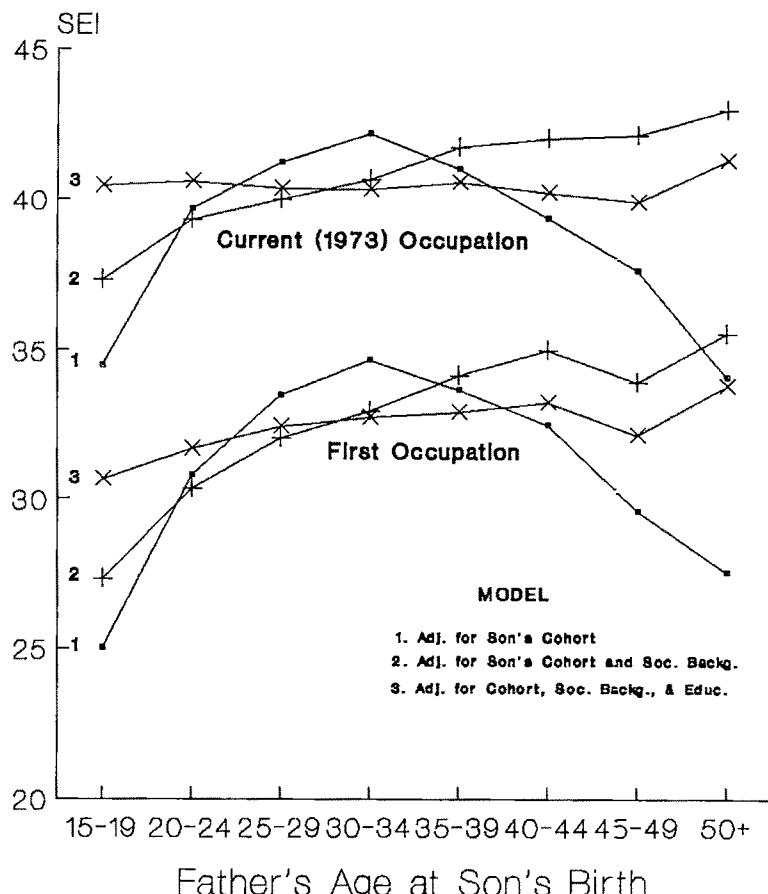


FIG. 3.—Observed and adjusted means of son's first and current occupations (SEI) by father's age at son's birth.

however, the effects of father's age on son's occupation may be transmitted entirely through the effects of father's age on son's schooling; that is, the net effects of father's age on son's occupation may be negligible. We include son's schooling in the regression model, which is reported for son's current occupation in the fifth and sixth columns of table 4. The corresponding adjusted means of son's occupations by age of father for both current and first occupations are presented in lines 3 in figure 3. Once son's educational attainment is taken into account, father's age has no net effect on son's current occupational status. The adjusted means under this model are approximately invariant with father's age, and the contrasts among father's age groups in sons' occupations are statistically insignificant. For son's first occupation, in contrast, once son's schooling is

TABLE 4

EFFECTS OF FATHER'S AGE, FAMILY BACKGROUND, AND EDUCATIONAL ATTAINMENT ON OCCUPATIONAL STATUS (SEI)
OF SON'S CURRENT (1973) OCCUPATION

VARIABLE	SELECTED MODELS					
	β	t	β	t	β	t
Constant	30.52	23.1	15.54	1.4	-4.84	-.5
Father's age at son's birth:						
15-19	5.19	4.3	1.99	1.9	.15	.2
20-24	6.76	5.8	2.65	2.6	-.10	-.1
25-29	7.72	6.6	3.33	3.2	-.14	-.2
30-34	6.54	5.5	4.40	4.2	.12	.1
35-39	4.89	3.9	4.70	4.2	-.23	-.2
40-44	3.16	2.2	4.85	3.9	-.53	-.5
45-49	-.35	-.2	5.67	4.7	.86	.7
50+						
Father's schooling						
Mother's schooling78	13.5	.03	.6	.03	.6
Family income49	17.1	.22	8.5	.22	8.5
Father's SEI18	20.9	.12	15.8	.12	15.8
Farm	-5.62	-14.1	-2.41	-6.8	-2.40	-6.8
Number of siblings	-.94	-17.4	-.26	-5.5	-.26	-5.6
Black	-7.12	-13.1	-6.41	-13.3	-6.40	-13.3
South	1.09	3.2	2.17	7.1	2.18	7.2
Son's schooling						
R ²243	4.04	79.7	4.03	.411

NOTE.—Analyses are based on 22,407 observations. All models include dichotomous variables that denote son's five-year birth cohort and whether data are present on each social background variable, except race and farm background, which have no missing data; t = ratio of coefficient to its standard error.

controlled, there remains a small positive net effect of father's age. Between the oldest and the youngest fathers, the status of son's first occupation varies by slightly more than three SEI points. At the point of entry into the occupational structure, therefore, sons with equivalent amounts of schooling differ slightly in the status of their positions according to their fathers' ages. These differences, however, soon dissipate.

The final two columns of table 4 report a regression model that includes both son's schooling and son's family background but excludes father's age at son's birth. In comparison with the models already discussed, these results show that inferences about effects of schooling and background on occupational status are unaffected by statistical controls for father's age at son's birth. Father's age contributes negligible explained variance to the model and fails to alter significantly any estimated coefficients once it is taken into account.

CONCLUSION

We find a substantial positive effect of father's age at son's birth on son's educational achievement. An observed curvilinear pattern of father's age effects is attributable to the adverse distribution of family background characteristics for the oldest parents, especially of mother's schooling and number of siblings. Once family background composition of fathers' age groups is taken into account, the effects of father's age on son's achievement is positive throughout all father's childbearing years. For son's occupational achievements, similar patterns of paternal age effects are observable, but, once the effect of son's educational attainment on his occupation are taken into account, net effects of father's age on son's occupation are negligible. The educational advantages that we observe for sons of older fathers are consistent with the view that life-cycle variation in socioeconomic welfare of parents affects offspring through differential timing of fertility. It is also consistent with the argument that the heavy and competing role demands of younger parents constrain the time and financial assets that they can provide their children. The effects of parents' ages on offspring's achievement may result from the different ages at which parents have a given child, or it may result from different ages at the *onset* of childbearing, which affect the welfare of children born subsequently. Without more detailed data, we cannot distinguish these effects.

Our research provides no direct information about the possible effects of mother's age on the welfare of offspring. Because ages of parents are highly correlated, the associations reported here may also reflect the ef-

fects of mother's age. We are also forced to restrict analyses to men who lived with both parents while they were growing up and have excluded men from female-headed families, who may be least affected by father's age. Further research is needed to see whether the life-cycle stage of each parent exerts an independent influence. Additionally, the relationships that we report are based on cohorts of men born between 1907 and 1951 and thus may not apply to recent cohorts of children who were born to teenage parents, to parents who have delayed their childbearing until their thirties, or to parents who have remarried and are building second families. That the positive effect of parents' ages is stable across the cohorts of men observed in the OCG survey (see n. 10) and consistent with research on smaller, yet more recent, samples of children (Hofferth 1987*b*), however, suggests that our findings are of contemporary as well as historical interest.

Our results for educational and occupational achievement indicate that specific effects of father's age on son's occupational status are negligible once son's schooling is taken into account, which suggests that multivariate analyses of processes of achievement are not seriously biased when the effects of father's age are ignored. However, son's educational and occupational achievements vary substantially with father's age at son's birth, suggesting that across societies or periods of time that vary considerably in the norms about the appropriate age to have children, both levels and distributions of socioeconomic achievements may vary.

Secular increases in parents' schooling and declines in average family size have been important sources of intercohort growth in average levels of educational attainment, despite rising costs and reduced benefits of college education (Mare 1981). The results reported here suggest yet another source of continued growth in schooling. Recent trends toward later fertility of American women mean higher average ages at which parents have children (Baldwin and Nord 1984), thus auguring higher levels of achievement among offspring than is implied by their other family-background characteristics alone.

Our results also reinforce previous research on the handicaps suffered by children born to teenage parents. Throughout the period covered by the OCG data, sons born to teenagers have averaged approximately 1.5 years of schooling less than sons born to older adults, a difference that holds even among children whose parents' educational attainments and other socioeconomic factors are equivalent. Although it is likely that this deficit is attributable to other aspects of family environment that are unmeasured in the present research, our results suggest that the handicaps faced by the offspring of young parents will be reduced but far from eliminated through improved levels of parental schooling.

American Journal of Sociology

REFERENCES

Alwin, Duane F., and Arland Thornton. 1984. "Family Origins and the Schooling Process." *American Sociological Review* 49:784-802.

Baldwin, Wendy H., and Christine Winquist Nord. 1984. "Delayed Childbearing in the U.S.: Facts and Fictions." *Population Bulletin* 39 (4): 1-42.

Becker, Gary S. 1975. *Human Capital*, 2d ed. New York: Columbia University Press.

Duncan, Otis Dudley. 1961. "A Socioeconomic Index for All Occupations." Pp. 109-38 in *Occupations and Social Status*, edited by A. J. Reiss, Jr. New York: Free Press.

Elder, Glen H., Jr. 1978. "Family History and the Life Course." Pp. 17-64 in *Transitions: The Family and the Life Course in Historical Perspective*, edited by T. K. Hareven. New York: Academic.

Espenshade, Thomas J. 1984. *Investing in Children: New Estimates of Parental Expenditures*. Washington, D.C.: Urban Institute.

Featherman, David L., and Robert M. Hauser. 1975. "Design for a Replicate Study of Social Mobility in the United States." Pp. 219-52 in *Social Indicator Models*, edited by K. C. Land and S. Spilerman. New York: Sage.

—. 1978. *Opportunity and Change*. New York: Academic.

Featherman, David L., Michael Sobel, and David Dickens. 1975. "A Manual for Coding Occupations and Industries into Detailed 1970 Categories and a Listing of 1970-Basis Duncan Socioeconomic and NORC Prestige Scores." Working Paper no. 75-1. University of Wisconsin-Madison, Center for Demography and Ecology.

Featherman, David L., and Kenneth I. Sprenner. 1988. "Class and the Socialization of Children: Constancy, Change, or Irrelevance?" Pp. 67-90 in *Child Development in Life-Span Perspective*, edited by E. M. Hetherington, R. M. Lerner, and M. Perlmuter. Hillsdale, N.J.: Erlbaum.

Gerson, Kathleen. 1985. *Hard Choices: How Women Decide about Work, Career, and Motherhood*. Berkeley and Los Angeles: University of California Press.

Hauser, Robert M. Forthcoming. "The Earnings Trajectories of Young Men." In *Social Stratification in Japan and the United States*, edited by D. J. Treiman and K. Tominaga.

Hauser, Robert M., and David L. Featherman. 1976. "Equality of Schooling: Trends and Prospects." *Sociology of Education* 49:99-120.

Hauser, Robert M., and William H. Sewell. 1985. "Birth Order and Educational Attainment in Full Sibships." *American Educational Research Journal* 22:1-23.

Hauser, Robert M., Shu-Ling Tsai, and William H. Sewell. 1983. "A Model of Stratification with Response Error in Social and Psychological Variables." *Sociology of Education* 56:20-46.

Hayes, Cheryl D. 1987. *Risking the Future: Adolescent Sexuality, Pregnancy, and Childbearing*, vol. 1. Washington, D.C.: National Academy.

Henretta, John C., and Richard T. Campbell. 1978. "Net Worth as an Aspect of Status." *American Journal of Sociology* 83:1204-23.

Hill, Martha S. 1985. "Patterns of Time Use." Pp. 133-76 in *Time, Goods, and Well-Being*, edited by F. T. Juster and F. P. Stafford. Ann Arbor: University of Michigan, Institute for Social Research.

Hofferth, Sandra L. 1984. "Long-Term Economic Consequences for Women of Delayed Childbearing and Reduced Family Size." *Demography* 21:141-55.

—. 1987a. "Social and Economic Consequences of Teenage Childbearing." Pp. 123-44 in *Risking the Future: Adolescent Sexuality, Pregnancy, and Childbearing*, vol. 2. Edited by S. L. Hofferth and C. D. Hayes. Washington, D.C.: National Academy.

—. 1987b. "The Children of Teen Childbearers." Pp. 174-206 in *Risking the*

Fathers and Sons

Future: Adolescent Sexuality, Pregnancy, and Childbearing, vol. 2. Edited by S. L. Hofferth and C. D. Hayes. Washington, D.C.: National Academy.

Hogan, Dennis P. 1981. *Transitions and Social Change*. New York: Academic.

Juster, F. Thomas. 1985. "Investments of Time by Men and Women." Pp. 177–204 in *Time, Goods, and Well-Being*, edited by F. T. Juster and F. P. Stafford. Ann Arbor: University of Michigan, Institute for Social Research.

Kerckhoff, Alan C. 1974. *Ambition and Attainment*. Washington, D.C.: American Sociological Association.

Mare, Robert D. 1979. "Social Background Composition and Educational Growth." *Demography* 16:55–72.

_____. 1980. "Social Background and School Continuation Decisions." *Journal of the American Statistical Association* 75:295–305.

_____. 1981. "Trends in Schooling: Demography, Performance, and Organization." *Annals of the American Academy of Political and Social Science* 453:96–122.

Mare, Robert D., and Meichu D. Chen. 1986. "Further Evidence on Sibship Size and Educational Stratification." *American Sociological Review* 51:403–12.

Mare, Robert D., Christopher Winship, and Warren N. Kubitschek. 1984. "The Transition from Youth to Adult: Understanding the Age Pattern of Employment." *American Journal of Sociology* 90:326–58.

Oppenheimer, Valerie Kincaide. 1982. *Work and the Family: A Study in Social Demography*. New York: Academic.

Taeuber, Karl E., and James A. Sweet. 1976. "Family and Work: The Social Life Cycle of Women." Pp. 31–60 in *Women and the American Economy: A Look to the 1980s*, edited by J. M. Kreps. Englewood Cliffs, N.J.: Prentice-Hall.

Tzeng, Meei-Shenn. 1987. "Fathers' Ages and Educational Mobility of Sons." M.S. thesis. University of Wisconsin—Madison, Department of Sociology.

Self-Perceptions of Black Americans: Self-Esteem and Personal Efficacy¹

Michael Hughes and David H. Demo

Virginia Polytechnic Institute and State University

This study examines the determinants of personal self-esteem, racial self-esteem, and personal efficacy in a 1980 national sample of black Americans. The findings show that the three dimensions are interrelated and each is anchored in interpersonal relations with family and friends. However, the three dimensions are produced by fundamentally different processes. Personal self-esteem is most strongly influenced by microsocial relations with family, friends, and community, while personal efficacy is generated through experiences in social statuses embedded in macrosocial systems of social inequality. We conclude that black self-esteem is insulated from systems of racial inequality, while personal efficacy is not, and suggest that this explains why black Americans have relatively high self-esteem but low personal efficacy. The belief that racial discrimination, rather than individual failure, accounts for low achievement among blacks is irrelevant to personal self-esteem and personal efficacy. In contrast, racial self-esteem is produced by a combination of education, interracial contact, and ideological processes.

Two major dimensions of self-perception known to be positively correlated are self-esteem and personal efficacy. Somewhat surprisingly, black Americans have relatively high self-esteem but relatively low personal efficacy. There is no widely accepted, empirically supported explanation for this apparent inconsistency. Most studies in this area have

¹ An earlier version of this paper was presented at the annual meeting of the American Sociological Association, Atlanta, Georgia, August 1988. We thank Clifford L. Brozman, Walter R. Gove, Bradley R. Hertel, Carolyn J. Kroehler, Kay Oehler, James W. Michaels, Terance D. Miethe, Richard D. Shingles, Dale W. Wimberley, Michael R. Wood, and anonymous *AJS* reviewers for comments on previous drafts. The data used in this paper were made available by the Inter-university Consortium for Political and Social Research. The data for the National Survey of Black Americans, 1979-1980, were originally collected by James S. Jackson and Gerald Gurin. Neither the collectors of the original data nor the consortium bears any responsibility for the analyses or interpretations presented here. Requests for reprints should be sent to Michael Hughes or David H. Demo, Department of Sociology, Virginia Polytechnic Institute and State University, Blacksburg, Virginia 24061.

focused on the issue of racial differences in self-esteem, paying particular attention to why black Americans do not generally have lower self-esteem than white Americans in spite of the fact that blacks experience low social status and economic and social discrimination in American society (see Porter and Washington [1979] and Wylie [1979] for reviews of this literature). These studies have also generated research questions about the processes involved in black self-concept development. In this article, we evaluate some of these questions by examining the determinants of black self-concept among respondents in a large national probability sample of black adults in the United States.

LITERATURE REVIEW

It is widely acknowledged that global self-esteem consists of two separate dimensions, one representing an individual's belief in his or her own virtue or moral worth, and the other indicating a sense of competence, efficacy, or personal control (Franks and Marolla 1976; Gecas 1982; Gecas and Schwalbe 1983).² Though conceptually distinct, these two dimensions are strongly related, so that those with high personal efficacy also have high self-esteem (Franks and Marolla 1976; Gecas and Schwalbe 1983). Studies comparing blacks with whites have shown that if we use whites as a standard, blacks have relatively high self-esteem but low personal efficacy.

Self-Esteem

Although studies of black self-esteem have concentrated on the issue of racial differences (black vs. white), interpretations of these studies have yielded arguments concerning the process of self-esteem development among blacks. Many empirical studies show that blacks have self-esteem equal to or greater than that of whites³ (see, e.g., Gordon 1969; Simmons

² In this article, we will refer to the *worth* dimension of self-esteem as *self-esteem* (or as *personal self-esteem* where it must be clearly distinguished from racial self-esteem) and the *efficacy* dimension of self-esteem as *personal efficacy*. The studies of self-esteem cited here use the Rosenberg self-esteem scale or some comparable measure of self-worth, unless otherwise noted. As Gecas (1982) has pointed out, Rosenberg's scale measures essentially the worth dimension of self-esteem rather than the efficacy dimension.

³ Studies of race and self-esteem exhibit a somewhat contradictory pattern (Wylie 1979; McCarthy and Yancey 1971; Rosenberg 1979; Porter and Washington 1979), with early studies arguing that being black is associated with poor self-esteem and later studies suggesting the opposite (for an analysis and debate over the meaning of these findings, see Adam [1978] and the commentaries by Simmons [1978] and Pettigrew [1978]).

American Journal of Sociology

et al. 1978; Taylor and Walsh 1979; Rosenberg and Simmons 1972). Such findings appear to contradict three accepted theoretical principles of self-esteem: reflected appraisals, social comparisons, and self-attribution.

The principle of *reflected appraisals* (Cooley 1902; Rosenberg 1979), which indicates that a person's self-esteem is a product of how that person believes others see him, suggests that persons belonging to low-status groups will internalize the negative evaluation of society and as a consequence have low self-esteem. According to the *social comparisons* principle (Festinger 1954), self-esteem is, in part, a consequence of individuals' comparing themselves with others and making positive or negative self-evaluations. If blacks experience low levels of social and economic achievement in American society and recognize their status in comparison with whites, this should lead to low self-esteem. The principle of *self-attribution* (Bem 1967) indicates that self-esteem results from persons' observing their own overt behavior and characteristics, especially successes and failures. This principle suggests that as blacks pursue goals valued by the average American but are deterred by discrimination, their experience of themselves as failures produces low self-esteem.

Rosenberg (1981, pp. 604–7; 1979, pp. 157–75), however, argues that the straightforward application of these principles to predicting low self-esteem for black Americans involves the following erroneous assumptions: (1) the reflected appraisals important to black self-esteem are those of the wider, largely white society rather than those of family, friends, teachers, and classmates in the black community; (2) blacks use whites as the standard in social comparisons; and (3) blacks attribute individual responsibility for the low status accorded black people in American society to themselves instead of blaming systematic racial discrimination.

Reflected appraisals, social comparisons, and black self-esteem.—For negative attitudes of whites toward blacks to affect black self-esteem, blacks must be aware of these attitudes, accept them, consider them significant, and believe them to be personally relevant. Rosenberg (1979, pp. 157–70) suggests that in most cases these conditions are not met and thus black self-esteem is unaffected by the attitudes of whites. Instead, among black and white students, self-esteem is strongly related to the reflected appraisals of parents, friends, and teachers (Rosenberg and Simmons 1972), and these sources of self-esteem are more important for blacks than for whites (Hoelter 1982).⁴

For similar reasons, comparisons with whites do not contribute

⁴ Further, because people selectively interact with others whom they perceive as having favorable attitudes toward them (Backman and Secord 1959, 1962; Dittes 1959), blacks who choose white friends are likely to choose those who hold more racially tolerant attitudes, and this should protect self-esteem.

Black Americans

significantly to black self-esteem. Rosenberg and Simmons (1972) argue that because adolescents compare themselves with others in their own immediate interpersonal environments, black high school students, who are usually surrounded by other blacks, not whites, tend to compare themselves with other blacks. Krause's (1983) study supports this conclusion, showing that interracial contact among students is irrelevant to black self-esteem.

Studies that support the principles of reflected appraisals and social comparisons have used children and adolescents as subjects. However, the social contexts in which reflected appraisals and social comparisons occur are different for children than they are for adults. For example, Rosenberg and Pearlin (1978) demonstrate that during the adult years social class has an important influence on self-esteem because reflected appraisals and social comparisons are generated in social relations conditioned by institutional inequality.⁵ Although whites dominate in social class and socioeconomic relations, black adults, particularly those who participate in the labor force, may play roles in institutional settings where reflected appraisals of the dominant white society and social comparisons with whites may be important for black self-esteem.

Self-attribution and black self-esteem.—In the late 1960s and early 1970s sociologists argued that black Americans were keenly aware that discrimination and the structure of opportunities were responsible for the failure of the average black person to enjoy a life comparable with those of typical whites (see, e.g., Gurin et al. 1969). Presumably, this belief protected black self-esteem by enabling blacks to attribute blame for failure to the social system rather than to themselves as individuals (McCarthy and Yancey 1971; Porter and Washington 1979).

However, there is little empirical support for this mechanism of self-esteem enhancement. Taylor and Walsh (1979) found that while blacks blamed the system more than whites, controlling for system-blame did not affect the relationship between race and self-esteem. More important, they found that system-blame was not related to positive self-esteem for either blacks or whites and, in fact, was weakly associated with poor self-esteem. However, sampling limitations make it unclear how relevant these findings are to an understanding of the experience of most blacks.

Religion and black self-esteem.—Religion has not been mentioned in the literature as an important institutional source of self-esteem; most discussions focus on family, friendship, school, and work. However, because the black church is a central institution in a community that is

⁵ Wylie (1979) has suggested that social class has a weak and inconsistent relationship with self-esteem. However, because race is a suppressor variable in the class/self-esteem relationship (Rosenberg and Pearlin 1978, p. 55), class may appear to be a weak predictor of self-esteem unless race is controlled.

American Journal of Sociology

isolated from the larger society (Nelsen and Nelson 1975; Blackwell 1985), the social relationships emanating from the church and associated with religion may be an important source of black self-esteem, just as they are a source of psychological well-being (Ortega, Crutchfield, and Rushing 1983). Mays and Nicholson's ([1933] 1971) empirical study of the black church illustrates the role of this institution in generating reflected appraisals, social comparisons, and self-attributions that enhance black self-esteem.

The opportunity found in the Negro church to be recognized, and to be "somebody," has stimulated the pride and preserved the self-respect of many Negroes who would have been entirely beaten by life, and possibly completely submerged. Everyone wants to receive recognition and feel that he is appreciated. The Negro Church has supplied this need. A truck driver of average or more than ordinary qualities becomes the chairman of the deacon board. A hotel man of some ability is the superintendent of the Sunday church school of a rather important church. A woman who would be hardly noticed, socially or otherwise, becomes a leading woman in the missionary society. . . . [Mays and Nicholson 1971, p. 289]

Summary: Self-esteem.—In sum, the literature suggests the following should be true for black adults: (1) quality of relationships with family and friends is positively related to self-esteem, (2) social contact with whites is generally unimportant to self-esteem, and (3) religious involvement is an important source of self-esteem. It is unclear how social class and system-blame are related to self-esteem.

Racial Self-Esteem

Another factor prominently mentioned in the literature as being important in black self-esteem is ethnic pride, or what Porter and Washington (1979) call *racial self-esteem*. While studies done before 1970, most involving children, consistently reported that blacks generally had low racial self-esteem, studies done in the past two decades report an increasing tendency for black children to prefer their own racial group (Hraba and Grant 1970; Katz and Zalk 1974). This trend suggests a subcultural explanation for racial self-esteem—that the black-consciousness movement socialized black children and young adults into accepting new norms and values (Porter and Washington 1979) and identifying more strongly with their group (Krystall et al. 1970; Toomer 1975).

This trend is also consistent with relative-deprivation theory (Pettigrew 1964, 1967). According to this view, when the rising expectations of blacks in the 1950s and 1960s were not fulfilled, feelings of fraternal deprivation led to militancy and an associated ideology involving high racial self-esteem (Porter and Washington 1979). Empirical studies report

that the most militant attitudes and highest ethnic pride are found in groups that combine high expectations and awareness of discrimination: the young, male, and middle class (Kronus 1971; Sampson and Milam 1975).

Though racial self-esteem is clearly important in its own right, most investigators predict that as racial self-esteem increases, personal self-esteem increases (Porter and Washington 1979, pp. 67-68), although estimates of the strength of this relationship vary (Rosenberg and Simmons 1972; Porter 1971; Gurin and Epps 1975). The most comprehensive study is Rosenberg's (1979, pp. 177-90) analysis of children and adolescents showing that group identification and pride have little bearing on personal self-esteem for racial or religious groups in contemporary America. This occurs largely because racial or religious identity is not as important as other identities. Unfortunately, the correlates of racial self-esteem have not been systematically examined among black adults.

Summary: Racial self-esteem.—In sum, the relatively small body of research on racial self-esteem suggests that the following should be true among black adults: (1) age and racial self-esteem are inversely related, (2) racial self-esteem is related to being male and being middle class, and (3) racial self-esteem has a positive relationship with personal self-esteem.

Personal Efficacy

Studies of race differences in personal efficacy find that blacks tend to score lower than whites (Coleman et al. 1966; Gordon 1969; Hunt and Hunt 1977). Evidence from surveys conducted in various years from 1958 to 1976 indicates that there was no change in the race difference in personal efficacy over this 18-year period, which included the civil rights movement and improvement in the social status of black Americans (Converse et al. 1980, pp. 7-19). As noted above, given that personal efficacy and self-esteem are positively correlated, and given that blacks have relatively high self-esteem, the fact that blacks have relatively low personal efficacy is something of an anomaly. Two possible explanations have been prominently mentioned.

First, personal efficacy and self-esteem may be uncorrelated or only weakly related among blacks (Hulbary 1975, p. 113; Crain and Weisman 1972, p. 81). If this were true, it would suggest that among blacks global self-esteem lacks an efficacy component and is largely a function of self-worth.

Second, low personal efficacy (i.e., high external control) among blacks may be a psychological reflection of low status resources and a recognition of widespread discrimination. "Feelings of external control may thus represent not a passive belief in chance or fate but instead system-blame,

which indicates a healthy sensitivity to the real world" (Porter and Washington 1979, p. 65; see also Hulbary 1975, p. 113). Researchers have argued that blaming the system, a reflection of external control, may contribute to high personal self-esteem (Pettigrew 1967; Porter and Washington 1979), thus explaining why blacks have both relatively high self-esteem and relatively low personal efficacy. A serious problem with this argument is that personal efficacy is distinguishable from, and unrelated to, system-blame (Gurin et al. 1969, pp. 36-41; Hulbary 1975, p. 113), which is, in turn, unrelated to self-esteem (Taylor and Walsh 1979).

Nonetheless, personal efficacy among blacks could be a product of social and economic inequality, quite independent of system-blame. Gecas and Schwalbe (1983; Gecas 1982; Schwalbe 1985) persuasively argue that the experience of effective performance is the most important factor in the development of a sense of personal efficacy. Because the social contexts that are particularly conducive to efficacious activity are institutional in nature—part of the macrostructure of society—indicators of an individual's location in that macrostructure (specifically, social class and work) should be strong predictors of personal efficacy.

Summary: Personal efficacy.—The literature suggests that the following should be true among black adults: (1) personal efficacy and self-esteem are positively related, and (2) social class is a strong predictor of personal efficacy.

THE PROBLEM

The literature on self-concept among black Americans offers no unambiguous answer to why blacks have relatively high self-esteem but low personal efficacy. A serious problem with previous studies is that their data are largely from samples of children and adolescents or restricted samples of adults. Because these samples are not representative of black adults and because some important variables (e.g., work and social class variables) may not have much effect until adulthood, it is unclear how the generalizations derived from these samples apply to the adult black population of the United States.

Our purposes in this study are to evaluate the generalizations presented in the summary sections above, using a national sample of black Americans, and to apply our findings about how social processes influence black self-perceptions toward understanding why black Americans have relatively high self-esteem but low personal efficacy.

We hypothesize that black self-esteem is largely a product of interpersonal relations in the family and community and is not strongly affected by dimensions of institutional inequality. This would explain why black

self-esteem is relatively high in spite of the fact that blacks experience discrimination and low social and economic status. An alternative hypothesis is that black self-esteem is dependent mostly on racial self-esteem or on blaming the system for low socioeconomic achievement.

We also hypothesize that personal efficacy among blacks is dependent on the experience of efficacious activity and is therefore strongly related to dimensions of institutional inequality such as occupational prestige, education, and income. Given the history of discrimination against blacks, this would explain why blacks have relatively low personal efficacy. The competing hypothesis is that personal efficacy is dependent on the degree of external control as reflected in perceptions of discrimination (i.e., system-blame).

DATA AND METHODS

The Sample

Data for this study come from the National Survey of Black Americans (NSBA) (Jackson and Gurin 1987). Jackson and Gurin (1987, p. i) provide the following sampling information: "The NSBA is a national probability household survey of 2,107 black Americans, 18 years of age and older, conducted in 1979 and 1980. . . . The 2,107 face to face household interviews were conducted by an all black, male and female professional interviewing staff, trained and supervised by the Survey Research Center, Institute for Social Research, The University of Michigan." The response rate was approximately 69%. The sample contains a "disparity in the proportion of women to men and a slight tendency to underrepresent younger people of both sexes and to overrepresent older women" (Jackson and Gurin 1987, p. iv). Other details concerning the data set and sampling procedure may be found in Jackson and Gurin (1987, pp. i-vii). The small sampling irregularities noted here do not affect the pattern of our findings. Analyses run within age and gender categories and those produced with a weighting scheme designed to correct for sampling errors regarding age and gender are substantively identical with the analyses presented below.

Because the processes responsible for self-concept formation occur over time, the optimal design would involve longitudinal data. Unfortunately, the NSBA is a cross-sectional study, and the results of our analysis should therefore be viewed with some caution. However, readers should also note that, with very few exceptions, the literature on self-concept formation is based on cross-sectional studies (Demo 1984) and that there are no large national samples of black adults from which self-perception data have been collected at different points in time.

Variables

Dependent variables.—(a) Self-esteem: Our six-item self-esteem scale includes two items from Rosenberg's (1979) self-esteem scale: "I feel that I'm a person of worth," and "I feel I do not have much to be proud of." Two items are from the self-esteem scale used in the Monitoring the Future Project (Bachman and Johnston 1978): "I feel that I can't do anything right," and "I feel that my life is not very useful." Two additional items were included to form a scale measuring the worth dimension of personal self-esteem: "I am a useful person to have around," and "As a person I do a good job these days." Four response categories ranging from "almost always true" to "never true" were provided for each statement. In a factor analysis of all six items, two factors emerge, one of positively worded items and the other of negatively worded items. This is typical of self-esteem scales that have both positively and negatively worded items (Carmines and Zeller 1979, pp. 62–70) and reflects response styles to such items. Before summing the scale, we recoded the negative items so that all items were in the positive direction. The α reliability coefficient for this scale is .66.

(b) Personal efficacy: The NSBA asked four questions measuring respondents' feelings of self-control and confidence in managing their own lives. These are the most highly correlated items (Wright 1976, p. 107) in a commonly used scale of personal efficacy (see Robinson and Shaver [1969, pp. 102–5] for a brief discussion of the scale and its history and validity). This scale has four items, each with two responses: (1) "Do you think it's better to *plan your life a good ways ahead*, or would you say life is *too much a matter of luck to plan ahead* very far?" (2) "When you do make plans ahead, do you usually get to *carry out things the way you expected*, or do *things usually come up to make you change your plans*?" (3) "Have you usually felt *pretty sure* your life would work out the way you want it to, or have there been times you *haven't been sure about it*?" (4) "Some people feel they *can run their lives* pretty much the way they want to, others feel the *problems of life are sometimes too big* for them." The α reliability coefficient for this scale is .57. Wright (1976) has shown that this measure is distinct from political efficacy, personal trust, and political trust. Our own analysis (not shown) using the same factor-analytic strategy employed by Wright (1976, pp. 105–9) indicates that this measure of personal efficacy is empirically distinct from our measure of personal self-esteem.

(c) Racial self-esteem: The operationalization of racial self-esteem is not standardized. We operationalize racial self-esteem for blacks as the belief that most black people possess positive characteristics and do not possess negative characteristics. The measurement amounts to an overall evalua-

tion of black people as a group. The question was worded: "How true do you think it is that most black people _____?" The question was completed with the following characteristics: (a) keep trying, (b) love their families, (c) are ashamed, (d) are lazy, (e) neglect their families, (f) are lying and trifling, (g) are hardworking, (h) do for others, (i) give up easily, (j) are weak, (k) are proud of themselves, (l) are honest, (m) are selfish, (n) are strong. Responses were coded (1) true, (2) somewhat true, (3) a little true, and (4) not true at all. Responses were recoded so that all items were in the positive direction. The α reliability coefficient for this scale is .80.

Social class and work variables.—(a) Socioeconomic status: As indicators of the respondent's location in the macrostructure of American society, we use years of education, personal income, and occupational prestige. The NSBA provides the 1970 census's three-digit occupation code. We assigned to each case the NORC and Duncan socioeconomic prestige score associated with the three-digit occupation codes (for details, see Featherman, Sobel, and Dickens 1975). We then calculated z-scores for both measures of occupational prestige and used each respondent's average score as his occupational prestige score. Analyses include this variable only for persons employed at the time of the interview.

(b) Job characteristics: Job security, expected mobility, self-perception of work quality, and job satisfaction also may have an effect on self-perception (Kohn and Schooler 1983; Mortimer and Lorence 1979). For employed respondents, we include indicators of the respondent's perceived chances of losing his job, perceived chances of being promoted, and self-rated job satisfaction. We also include an item measuring self-rated job performance that was asked of all respondents.

(c) Employment: As employment itself may contribute to positive self-evaluation, we include a dummy variable to distinguish those who have a current job from all others (1 = not employed).

Family, friendships, and religion.—Research indicates that, as is the case for children, the quality of family relations is important to self-esteem among adults (Demo, Small, and Savin-Williams 1987). Because adults spend considerable amounts of time in extrafamilial contexts, however, it is also important to examine the nature and quality of their informal relationships with friends. The four-item factor-weighted scale contains two items assessing the closeness of and satisfaction with family relations and two items assessing the quality of family and friendship roles.⁶ Factor analysis extracted a single factor with an eigenvalue of

⁶ An anonymous reviewer of a previous version of this paper raised two related questions about the appropriateness of including the two role items "Being a good friend" and "Taking care of your family's wants and needs" in the quality of family and friendship relations scale. First, although the items concerning closeness and satisfac-

1.57, accounting for 39.5% of the variance in the four items. Four response categories were provided for each question (factor loadings are in parentheses): (1) "Would you say your family members are *very close* in their feelings to each other, *fairly close*, *not too close*, or *not close at all?*" (.530); (2) "How satisfied are you with your family life, that is, the time you spend and the things you do with members of your family?" (.695); (3) "Given the chances you have had, how well have you done in taking care of your family's wants and needs?" (.685); (4) "Given the chances you have had, how well have you done at being a good friend?" (.587).

To measure religious involvement, we combined three items into one scale using factor analysis to weight each item. The factor analysis extracted a single factor with an eigenvalue of 1.90, accounting for 63.3% of the variance in the three items. The items and the factor loadings for each item are as follows: (1) "How often do you usually attend religious services?" (.804); (2) "How religious would you say you are?" (.781); (3) "How often do you read religious books or other religious materials?" (.800).

Ethnicity and racial ideology variables.—(a) System-blame and inter-racial contact: Our measure of system-blame is an additive scale of two items. Respondents were asked to respond to two questions with two possible choices: (1) "In the United States, if black people don't do well in

tion with family life may be good measures of the quality of interpersonal relations, the two role items may not be and therefore may not belong in the scale. Second, the two role items may be measuring the same thing as some items in the self-esteem scale (e.g., "I am a useful person to have around"), and thus any relationship found between self-esteem and quality of family and friendship may be an artifact of measurement. We argue that the two items in question measure the quality of role relationships that are particularly important in organizing the interpersonal behaviors that affect self-esteem through reflected appraisal and social comparison processes. Empirically, we find that when items from both scales are included in the same factor analysis, the four items in the quality of family and friendship scale load on a single factor, distinct from the self-esteem scale items. We believe that the analyses here that use the four-item quality of family and friendship scale provide the best representation possible with these data of the effects of close interpersonal relations on self-esteem. We emphasize, however, that neither the pattern of findings nor the theoretical implications of the present study are dependent on including the two role-quality items, as we have done in our analyses. In analyses not presented here, we found the following: (1) when we entered the quality of family and friendship items in a set of regressions as separate variables and evaluated the effect of different blocks of theoretically relevant variables through the proportion of variance each contributed independently to the variance explained, this more cumbersome analysis proved to be substantively identical with that presented below, and (2) when we reduced the quality of family and friendship scale to two items by eliminating the items about being a good friend and meeting one's family's needs and performed regressions analogous to those below, we found the same theoretically significant patterns of findings, including significant ($P < .001$) effects of the reduced two-item scale.

Black Americans

life it is because: one, they don't work hard to get ahead, *or* two, they are kept back because of their race"; and (2) "In this country, if black people do not get a good education or job, it is because: one, they haven't had the same chances as whites in this country, *or* two, they have no one to blame but themselves." The correlation between the two items is .370. A high score indicates that discrimination (or the system) is blamed.

The measure of interracial contact indicates the level of involvement the respondent has had with white people over his lifetime. Respondents were asked to judge the racial composition of eight social settings in their lives: (1) grammar or elementary school, (2) junior high school, (3) high school, (4) college, (5) neighborhood while growing up, (6) present neighborhood, (7) church or place of worship usually attended, and (8) present workplace, if employed. Responses were coded (1) all blacks, (2) mostly blacks, (3) about half blacks, (4) mostly whites, and (5) almost all whites and were summed across the seven items. There are considerable missing data on these items because many respondents had not been involved in all the settings indicated; for example, some grew up in areas where junior high schools do not exist. Where appropriate, we replaced missing data with data regarding analogous settings from the same life-cycle stage.

(b) Black-consciousness variables: We also include two measures of black consciousness: black identity and black separatism. The measure of black identity used in the present study was developed by Broman, Neighbors, and Jackson (1988) for use in their study of racial group identification among black adults. Respondents were asked: "How close do you feel in your ideas and feelings about things to black people who are _____?" The kinds of people asked about were (a) poor, (b) religious, (c) young, (d) middle class, (e) working class, (f) older, (g) elected officials, and (h) professional black people. This question yielded eight items, each coded (1) not close at all, (2) not too close, (3) fairly close, (4) very close. Scores were averaged for each respondent. The α reliability coefficient for the scale is .82. If responses were missing on three or fewer of the eight items, the mean response of the items with valid responses was entered for the missing data. Scale scores were then calculated by summing item scores for those respondents with sufficient data for inclusion in the analysis.

Regarding black separatism, a number of questions were asked concerning commitment to African culture and the degree to which blacks should confine some of their social relationships to other blacks. Respondents were asked if they agreed with the following: (1) black children should learn an African language, (2) blacks should always vote for black candidates when possible, (3) black women should not date white men, (4) black people should shop in black-owned shops whenever possible,

(5) black men should not date white women, (6) black parents should give their children African names. Four responses were possible, ranging from "strongly disagree" to "strongly agree." Since the two questions on dating were very strongly correlated, they were combined into a single item by averaging the responses on these two. The resulting five items were averaged for each respondent. In a factor analysis these items load strongly on a single factor and yield an α reliability coefficient of .61. If responses were missing on two or fewer of the five items, the mean response on the items with valid responses for each respondent was entered for the missing data.

Control variables.—Age and gender are included as control variables in the analyses that appear below because some of the independent variables, particularly social class and work variables, are related to both age and gender.

FINDINGS

Regression analysis was used to evaluate the relative effects of various determinants of racial self-esteem, personal efficacy, and personal self-esteem. A complete matrix of means, standard deviations, and correlations among variables is presented in the Appendix.

Racial Self-Esteem

In table 1 we present zero-order correlations and regression analyses for racial self-esteem for all respondents and for those currently employed. Separate analyses are presented because some of the theoretically significant independent variables (i.e., occupational prestige of current job, chances of losing one's job, chances of being promoted, and job satisfaction) are relevant only for those who are currently employed. The first regression analysis includes all predictor variables, and a second regression analysis includes only variables that are significant at .05 or less in the original regression analysis. Not surprisingly, the most important predictors of racial self-esteem are the ethnicity and racial ideology variables. System-blame, black identity, black separatism, and interracial contact are all positively, and significantly, related to racial self-esteem in both regression analyses. Quality of family and friendship relations is significant in the analysis for all respondents, occupational prestige is significant among the employed, and education is significant in both analyses.

In sum, racial self-esteem is enhanced by black consciousness: system-blame, black identity, and black separatism. Also important are contact with whites, positive relationships with family and friends, education,

TABLE 1

REGRESSION ANALYSIS FOR RACIAL SELF-ESTEEM

DETERMINANT	<i>r</i>	ALL RESPONDENTS		EMPLOYED RESPONDENTS	
		All Variables	All Significant Variables	<i>r</i>	All Variables
<i>Social class and work:</i>					
Education	.075**	.065*	.085**	.134***	.091*
Occupational prestige	.093***142***	.090*
Personal income048097**	.019
Likely lose job	-.076*	-.073*
Promotion chances (low)	-.020	.010
Job performance	.046*	-.011092**	.034
Job satisfaction	-.021	-.055
Employment status (not employed)	-.068***	-.030
<i>Family and religion:</i>					
Family and friendship	.124***	.108***	.100***	.099**	.061
Religious involvement	.022	-.032028	-.052
<i>Ethnicity and racial ideology:</i>					
System-blame	.161***	.140***	.139***	.183***	.142***
Interracial contact	.092***	.109***	.117***	.086**	.091*
Black identity	.120***	.108***	.077**	.112***	.138***
Black separatism	.103***	.092***	.087***	.105***	.108**
<i>Control variables:</i>					
Gender (female)	-.033	.013	-.017	.015	.033
Age	.012	.008	.010	.011	.016
R ²076***	.065***	.098***	.082***
N	1,609	1,698	1,879	847	886
					1,009

Note.—Columns headed *r* contain zero-order correlation coefficients. All other coefficients are standardized regression coefficients. "..." = variable not included in regression.

**P* ≤ .05.

***P* ≤ .01.

****P* ≤ .001.

American Journal of Sociology

and occupational prestige. Though significant, the social class variables are not as strong as racial ideology variables. Religious involvement, age, gender, employment, and job characteristics have no independent relationships with racial self-esteem.

Personal Efficacy

Table 2 contains the analyses for personal efficacy. In the multiple regression analyses, the significant predictors among all respondents are education, personal income, quality of family and friendship relations, gender, and age. The same general pattern holds for the employed, for whom occupational prestige and self-rating of job performance are also significant, but gender is not.

Taken together, the measures of socioeconomic status are the most important predictors of personal efficacy relative to other variables. In the regression analyses, the ethnicity and racial ideology variables, religious involvement, and job characteristics are unrelated to personal efficacy. Overall, the findings indicate that higher socioeconomic status, better-quality relations with family and friends, being male, and being older are related to a greater sense of efficacy. When we add racial self-esteem to the analyses as an independent variable, we find that it is a significant predictor of personal efficacy and that its inclusion does not change the pattern of the findings.

Self-Esteem

Regression analyses for personal self-esteem are in table 3. Consistent with our predictions, the strongest influences on self-esteem among all respondents and among the currently employed are quality of family and friendship relations and religious involvement. Weaker but significant predictors for all respondents are education, personal income, self-rating of job performance, employment status, and being male. Among those currently employed, the only social class or work variable to exert a significant influence is self-rated job performance.

When we add racial self-esteem to the analysis, we find that it is a statistically significant predictor, and its inclusion does not change the pattern of findings. When we include personal efficacy in the analysis of all respondents, education and self-rated job performance become nonsignificant, but quality of family and friendship relations and religious involvement remain strong. In both analyses, personal efficacy is strongly related to self-esteem, even with controls for other important variables.

Notably, though black identity has a significant zero-order relationship with self-esteem, this correlation disappears in the regression analysis.

TABLE 2
REGRESSION ANALYSIS FOR PERSONAL EFFICACY

DETERMINANT	r	ALL RESPONDENTS			EMPLOYED RESPONDENTS		
		All Variables	All Significant Variables	All Significant Variables + Racial Self-Esteem	r	All Variables	All Significant Variables
Social class and work:							
Education152***	.184***	.203***	.196***	.263***	.186***	.182***
Occupational prestige211***	.123***	.114***	.105***	.258***	.123**	.111**
Personal income238***	.086*	.093*
Likely lose job	—.081**	—.044	—.097**
Promotion chances (low)	—.097**	—.044	—.126***
Job performance097***	.018126***	.079*	.062
Job satisfaction005	—.051	—.051
Employment status (not employed)	—.076***	.016
Family and religion:							
Family and friendship146***	.134***	.138***	.123***	.084**	.074*	.083*
Religious involvement063**	.029032	.012	.070*
Ethnicity and racial ideology:							
System-blame030	.005053	.014
Interracial contact073**	.041108***	.030
Black identity000	—.039	—.047	—.036
Black separatism013	.043	—.022	.039
Control variables:							
Gender (female)	—.098***	—.065*	—.061*	—.062*	—.054	—.050	—.058
Age093***	.157***	.159***	.159***	.051	.125**	.103**
Self-perception:							
Racial self-esteem158***117***	.189***	.149***
R^2098***	.093***	.107***139***	.113***
N	1,609	1,638	1,740	1,735	847	835	967

Note.—Columns headed r contain zero-order correlation coefficients. All other coefficients are standardized regression coefficients. " . . ." = variable not included in regression.

* $P \leq .05$.

** $P \leq .01$.

*** $P \leq .001$.

TABLE 3
REGRESSION ANALYSIS FOR PERSONAL SELF-ESTEEM

DETERMINANT	r	ALL RESPONDENTS				EMPLOYED RESPONDENTS			
		All Variables		Significant Variables		All Variables		Significant Variables	
		All	Significant	All	Significant	All	Significant	All	Significant
Social class and work:									
Education003†††	.078†††	.000***	.079***	.017	.011	.058		
Occupational prestige062*	.030		
Personal income119***	.056*	.059*	.053	.030	.056	.028		
Likely lose job	-.104***	-.036		
Promotion chances (low)	-.059*	.003		
Job performance199***	.065***	.061*	.058*	.046	.228***	.110***	.093**	.086**
Job satisfaction104***	.001		.067*
Employment status (not employed)	-.141***	-.087***	-.082***	-.078**	-.086***

Family and religion:							
Family and friendship	.326***	.271***	.285***	.274***	.248***	.292***	.215***
Religious involvement	.214***	.158***	.155***	.156***	.158***	.228***	.214***
Ethnicity and racial ideology:							
System-blame	-.010	-.012					
Interracial contact	.005	.029					
Black identity	.141***	.006					
Black separation	.051*	.042					
Control variables:							
Gender (female)	-.032	-.044	-.048*	-.047*	-.030	-.002	-.049
Age	.076***	.007	-.003	-.006	-.042	.099***	-.003
Self-perception:							
Racial self-esteem	.180***		.113***	.090***	.178***		.136***
Personal efficacy	.298***		.162***	.166***	.179***	.216***	.279***
R ²							
N	1,609	1,672	1,754	1,750	1,680	847	877

Note.—Columns headed r contain zero-order correlation coefficients. All other coefficients are standardized regression coefficients. “. . .” = variable not included in regression.

* $P \leq .05$.

** $P \leq .01$.

*** $P \leq .001$.

American Journal of Sociology

This happens because the correlations between black identity and religious involvement (.344) and between black identity and quality of family and friendship relations (.354) are substantial. Regressing self-esteem on religious involvement, quality of family and friendship relations, and black identity (analysis not shown), we found that the standardized regression coefficient for black identity reduced to $-.009$ or essentially no effect. We conclude that religious involvement and quality of family and friendship relations produce both strong black identity and high self-esteem and that this explains the zero-order relationship between black identity and self-esteem.

Interaction of System-Blame and Social Class

Because of recent evidence indicating that the psychological centrality of social class for self-esteem increases with age (Demo and Savin-Williams 1983; Rosenberg and Pearlin 1978), we were surprised by the weak relationship between social class variables and self-esteem among black American adults, particularly among the currently employed. One plausible explanation for these findings is that black people may generally attribute individual success and failure to a discriminatory system beyond their control, thereby rendering social class irrelevant to black self-esteem. While our findings indicate that system-blame itself is not related to self-esteem, it may statistically interact with social class. For those who blame the system, social class should not be psychologically central to self-esteem and thus should be unrelated or even negatively related to self-esteem. For those who attribute responsibility to individuals, however, social class should be psychologically central to self-esteem, reflecting success or failure in life, and thus be positively related to self-esteem.

To test this hypothesis, we constructed an interaction term by multiplying a socioeconomic-status index (a weighted index of education, personal income, and occupational prestige) by a dummy variable indicating individual versus system-blame (1 = blamed the system on either of the two variables in the system-blame index, and 0 = blamed the individual). The system-blame dummy variable, the socioeconomic-status index, and this interaction term were entered in a regression analysis predicting self-esteem (results not shown). Other variables in the equation were quality of family and friendship relations, religious involvement, self-rated job performance, age, and gender. The regression coefficient for the interaction term was neither significant (at .05 or less) nor in the direction suggested above. Social class, we conclude, is not psychologically central to self-esteem among blacks, not even among those who believe that achievement is due to individual efforts.

DISCUSSION

Our findings have a number of implications for theoretical discussions and research on self-concept formation among black Americans.

Principles of Self-Esteem

In theoretical terms, our findings strongly support two of Rosenberg's hypotheses (1979, pp. 157-75; 1981, pp. 604-7) concerning the application of self-esteem principles to black Americans. First, that the family/friendship and religious-involvement variables are strong predictors of self-esteem and social class and work variables are weak ones indicates that the reflected appraisals important to black self-esteem are those of family, friends, and community members, not those of the wider world of institutional inequality dominated by whites. The importance of religion also provides strong empirical support for the positive influence of the black church postulated by Mays and Nicholson (1971) and Nelsen and Nelsen (1975). Second, consistent with Krause (1983), we found that interracial contact is not related to black self-esteem, supporting the idea that social comparisons with whites are not relevant to black self-esteem.

We found no support for the hypothesis that high black self-esteem is due to blaming discrimination for low socioeconomic achievement. This finding corroborates Taylor and Walsh (1979) and is particularly important given the widespread assumption that system-blaming leads to a more positive self-image among blacks (Myrdal 1944, p. 759; McCarthy and Yancey 1971, pp. 667-68; Porter and Washington 1979; Rosenberg 1981, p. 601; Shingles 1981, p. 78). We interpret these findings as indicating that system-blame represents a political belief concerning discrimination and individual responsibility that is not internalized in a way that is relevant for self-esteem.

Psychological Centrality of Social Class

Social class, shown by Rosenberg and Pearlin (1978) to be psychologically central to self-esteem for white male adults, appears not to be psychologically central to self-esteem among black adults. The socioeconomic status and work variables have weak or nonexistent relationships with self-esteem among blacks. When we examined the interaction between system-blame and socioeconomic status, we found no evidence that socioeconomic status had any relationship to self-esteem among those who believe that individual failure (and, by implication, success) is attributable to efforts of individuals. However, our analyses do not indicate that socio-

American Journal of Sociology

economic status is irrelevant to black self-esteem. The weak but significant effect of education on self-esteem disappears when we enter personal efficacy. This occurs, we argue, because education produces greater personal efficacy, which is strongly related to self-esteem, not because it provides an intrinsic sense of self-worth.

In 1971, McCarthy and Yancey hypothesized that social class would have a weaker relationship with self-esteem among blacks than among whites (pp. 666, 668). Their argument was that middle-class blacks tend to accept white achievement values and therefore blame themselves for failures, while lower-class blacks can blame discrimination. Taken together, our finding that social class is not related to self-esteem among black adults and Rosenberg and Pearlin's (1978) finding of a significant relationship for white adult males provide some support for McCarthy and Yancey's hypothesis. However, the fact that system-blame is irrelevant in predicting self-esteem casts doubt on McCarthy and Yancey's explanation. Instead, we argue that the history of discrimination has had widespread effects throughout the entire black population, affecting the unconscious selection of dimensions on which self-esteem is based.

Historically, high levels of unemployment and racial discrimination have meant that the institutionalized work roles available to blacks have not been particularly stable. If it is true, as Zurcher (1977) argues, that when institutionalized roles are unstable they are less reliable sources of self-definition, then it is reasonable that social class and work would not be psychologically central to self-esteem among blacks. Studies have demonstrated that social roles that are not conducive to self-esteem are selectively devalued (Rosenberg 1979) and that "people gravitate toward opportunity structures that offer support for their self-conceptions" (Swann 1983, p. 36). Rather than being dependent on high socioeconomic attainment for a strong sense of self-worth, black self-esteem is anchored in interpersonal relations with extended families, friends, and church-based social networks. If there are sharp declines in racial discrimination in employment and in the perceptions among black people that these disadvantages exist, then social class and work should become more important for black self-esteem.

Racial Self-Esteem

Our analysis of racial self-esteem contradicts the subcultural explanation that young blacks identify more closely with their ethnic group (Krystall et al. 1970; Toomer 1975). Apparently, recent socialization practices are no more or less effective than earlier practices in teaching group pride among blacks.

Not surprisingly, racial ideology variables that reflect cultural pro-

Black Americans

cesses internal to the black community are the most important predictors of racial self-esteem. Quality of family and friendship relations also has a modest effect. However, racial self-esteem is not determined entirely by intraethnic, in-group processes. The effects of education and contact with whites suggest that integration into the wider society leads blacks to appreciate characteristics of their own ethnic group. If we assume that education and interracial contact lead to high achievement expectations, our findings support the relative-deprivation explanation proposed by Porter and Washington (1979, p. 58). Those with the highest racial self-esteem are those who combine high expectations with ethnic identity and the understanding that black achievement in the wider society has been blocked by racial discrimination.

Racial self-esteem is a significant predictor of both personal efficacy and self-esteem. Although other variables are more important, this finding supports Porter and Washington's (1979) position that, for black Americans, group pride is an important factor in self-perception.

Personal Efficacy

Personal efficacy is not associated with the belief that one's fate is controlled by a system that discriminates on the basis of race. Instead it is a product of one's location in the social order. These findings suggest, contrary to much recent speculation, that institutional inequality has important effects on black self-perceptions. Specifically, although social class is largely unrelated to personal self-esteem, inequality strongly influences personal efficacy by depriving blacks of opportunities that would enable them to feel efficacious (Franks and Marolla 1976).⁷ We interpret the strong association between social class variables and personal efficacy as support for Gecas and Schwalbe's (1983) hypothesis that the experience of effective performance is the most important factor in the development of personal efficacy. Discrimination in institutional life has largely relegated blacks to subordinate positions and excluded them from positions of power, resources, and contexts of action that afford individuals the best opportunities to experience themselves as powerful and autonomous. This is compelling support for Adam's (1978) assertion that "the effects of a racist social structure" have not simply faded away (p. 49). Informal family, friendship, and community social relations also are significantly

⁷ It is also necessary to recognize the reciprocal nature of relationships between social structure and personality. It is certainly plausible, e.g., that among blacks personal efficacy influences educational and occupational attainment. Although the magnitude of such reciprocal effects is generally weaker than that of social structural effects on personality (House 1981; Kohn and Schoeller 1983), the bidirectional quality of the processes must be recognized and provides an important focus for future research.

American Journal of Sociology

related to personal efficacy but are less important than institutional inequality.

CONCLUSION

This study underscores the importance of researching black self-concept in a multidimensional framework. Racial self-esteem, personal efficacy, and personal self-esteem are interrelated, and, although each dimension is firmly anchored in the quality of relations with family and friends, the three dimensions are largely generated through different micro- and macrosocial processes that vary in their relations to institutional inequality. Personal self-esteem is generated in microprocesses in the black community that are insulated from institutional inequality. Racial self-esteem is partly generated by cultural and interpersonal processes internal to the black community, but it is also promoted by institutional inequality through education, interracial contact, and the perception that discrimination impedes the achievement of black people. Personal efficacy, produced by the personal experience of efficacious activity, is strongly affected by one's location in the macro-order of institutional inequality. The formal and informal processes of discrimination that exclude black people from positions of power and authority in American society also impair the development of personal efficacy.

Our findings strongly suggest that the apparent anomaly of high self-esteem coexisting with low personal efficacy among black Americans is understandable when we take account of the fact that inequality has little effect on black self-esteem but is an important determinant of personal efficacy. Perhaps it is time we turned away from the issue of self-esteem among black Americans and focused instead on personal efficacy, the social-psychological characteristic most affected by racial inequality in this society.

REFERENCES

Adam, Barry D. 1978. "Inferiorization and Self-Esteem." *Social Psychology* 41:47-53.
Bachman, Jerald G., and L. D. Johnston. 1978. *The Monitoring the Future Project: Design and Procedures*. Ann Arbor: University of Michigan, Institute for Social Research.
Backman, C. W., and P. F. Secord. 1959. "The Effect of Perceived Liking on Interpersonal Attraction." *Human Relations* 12:379-84.
_____. 1962. "Liking, Selective Interaction, and Misperception in Congruent Interpersonal Relations." *Sociometry* 25:321-35.
Bem, Daryl J. 1967. "Self-Perception: An Alternative Interpretation of Cognitive Dissonance Phenomena." *Psychological Review* 74:183-200.

Black Americans

Blackwell, James E. 1985. *The Black Community: Diversity and Unity*, 2d ed. New York: Harper & Row.

Broman, Clifford L., Harold W. Neighbors, and James S. Jackson. 1988. "Racial Group Identification among Black Adults." *Social Forces* 67:146-58.

Carmines, Edward G., and Richard A. Zeller. 1979. "Reliability and Validity Assessment." Sage University Paper Series on Quantitative Applications in the Social Sciences, no. 07-017. Beverly Hills, Calif.: Sage.

Coleman, James, E. Campbell, J. McPartland, et al. 1966. *Equality of Educational Opportunity*. Washington, D.C.: Government Printing Office.

Converse, Philip, Jean D. Dotson, Wendy J. Hoag, and William H. McGee. 1980. *American Social Attitudes Data Source Book: 1947-1978*. Cambridge: Harvard University Press.

Cooley, Charles H. 1902. *Human Nature and the Social Order*. New York: Scribner's.

Crain, Robert L., and Carol Sachs Weisman. 1972. *Discrimination, Personality, and Achievement*. New York: Academic.

Demo, David H. 1984. "The Self over Time." Paper presented at the annual meeting of the American Sociological Association, San Antonio.

Demo, David H., and Ritch C. Savin-Williams. 1983. "Early Adolescent Self-Esteem as a Function of Social Class: Rosenberg and Pearlin Revisited." *American Journal of Sociology* 88:763-74.

Demo, David H., Stephen A. Small, and Ritch C. Savin-Williams. 1987. "Family Relations and the Self-Esteem of Adolescents and Their Parents." *Journal of Marriage and the Family* 49:705-15.

Dittes, J. E. 1959. "Attractiveness of Group as a Function of Self-Esteem and Acceptance by Group." *Journal of Abnormal and Social Psychology* 59:77-82.

Featherman, David L., Michael Sobel, and David Dickens. 1975. "A Manual for Coding Occupations and Industries into Detailed 1970 Categories and a Listing of 1970-Basis Duncan Socioeconomic and NORC Prestige Scores." Working paper 75-1. University of Wisconsin—Madison, Center for Demography and Ecology.

Festinger, Leon. 1954. "A Theory of Social Comparison Processes." *Human Relations* 7:117-40.

Franks, David D., and Joseph A. Marolla. 1976. "Efficacious Action and Social Approval as Interacting Dimensions of Self-Esteem: A Tentative Formulation through Construct Validation." *Sociometry* 39:324-41.

Gecas, Viktor. 1982. "The Self-Concept." Pp. 1-33 in *Annual Review of Sociology*, vol. 8. Palo Alto, Calif.: Annual Reviews.

Gecas, Viktor, and Michael L. Schwalbe. 1983. "Beyond the Looking-Glass Self: Social Structure and Efficacy-based Self-Esteem." *Social Psychology Quarterly* 46:77-88.

Gordon, Chad. 1969. *Looking Ahead*. Washington, D.C.: American Sociological Association.

Gurin, P., and E. Epps. 1975. *Black Consciousness, Identity, and Achievement*. New York: Wiley.

Gurin, P., G. Gurin, R. Lao, and M. Beattie. 1969. "Internal/External Control and the Motivational Dynamics of Negro Youth." *Journal of Social Issues* 25:29-53.

Hoelter, Jon W. 1982. "Race Differences in Selective Credulity and Self-Esteem." *Sociological Quarterly* 23:527-37.

House, James S. 1981. "Social Structure and Personality." Pp. 525-61 in *Social Psychology: Sociological Perspectives*, edited by Morris Rosenberg and Ralph H. Turner. New York: Basic.

Hraba, J., and G. Grant. 1970. "Black Is Beautiful: A Reexamination of Racial Preference and Identification." *Journal of Personality and Social Psychology* 16:398-402.

American Journal of Sociology

Hulbary, William. 1975. "Race, Deprivation, and Adolescent Self-Image." *Social Science Quarterly* 56:105-14.

Hunt, Janet G., and Larry L. Hunt. 1977. "Racial Inequality and Self-Image: Identity Maintenance and Identity Diffusion." *Sociology and Social Research* 61:539-59.

Jackson, James S., and Gerald Gurin. 1987. *National Survey of Black Americans, 1978-80* (machine readable codebook). Ann Arbor, Mich.: Inter-university Consortium for Political and Social Research, Institute for Social Research.

Katz, P., and S. Zalk. 1974. "Doll Preferences: An Index of Racial Attitudes." *Journal of Educational Psychology* 66:663-68.

Kohn, Melvin L., and Carmi Schooler. 1983. *Work and Personality: An Inquiry into the Impact of Social Stratification*. Norwood, N.J.: Ablex.

Krause, Neal. 1983. "The Racial Context of Black Self-Esteem." *Social Psychology Quarterly* 46:98-107.

Kronus, S. 1971. *The Black Middle Class*. Columbus, Ohio: Merrill.

Krystall, E., N. Friedman, G. Howze, and E. Epps. 1970. "Attitudes toward Integration and Black Consciousness: Southern Negro High-School Students and Their Mothers." *Phylon* 31:104-13.

McCarthy, J. D., and W. L. Yancey. 1971. "Uncle Tom and Mr. Charlie: Metaphysical Pathos in the Study of Racism and Personal Disorganization." *American Journal of Sociology* 76:648-72.

Mays, Benjamin E., and Joseph W. Nicholson. (1933) 1971. "The Genius of the Negro Church." Pp. 287-91 in *The Black Church in America*, edited by Hart M. Nelsen, Raytha L. Yokley, and Ann K. Nelsen. New York: Basic.

Mortimer, Jeylan T., and Jon Lorence. 1979. "Occupational Experience and the Self-Concept: A Longitudinal Study." *Social Psychology Quarterly* 42:307-23.

Myrdal, Gunnar. 1944. *An American Dilemma*. New York: Harper.

Nelsen, Hart M., and Anne Kusener Nelsen. 1975. *Black Church in the Sixties*. Lexington: University Press of Kentucky.

Ortega, Suzanne T., Robert D. Crutchfield, and William A. Rushing. 1983. "Race Differences in Elderly Personal Well-Being." *Research on Aging* 5:101-18.

Pettigrew, Thomas F. 1964. *Profile of the Negro American*. Princeton, N.J.: Van Nostrand.

—. 1967. "Social Evaluation Theory: Convergences and Applications." *Nebraska Symposium on Motivation* 15:241-311.

—. 1978. "Placing Adam's Argument in a Broader Perspective." *Social Psychology* 41:58-61.

Porter, Judith R. 1971. *Black Child, White Child: The Development of Racial Attitudes*. Cambridge: Harvard University Press.

Porter, Judith R., and Robert E. Washington. 1979. "Black Identity and Self-Esteem: A Review of Studies of Black Self-Concept, 1968-1978." Pp. 53-74 in *Annual Review of Sociology*, vol. 5. Palo Alto, Calif.: Annual Reviews.

Robinson, John P., and Phillip R. Shaver. 1969. *Measures of Social Psychological Attitudes*. Ann Arbor: University of Michigan, Institute for Social Research.

Rosenberg, Morris, 1979. *Conceiving the Self*. New York: Basic.

—. 1981. "The Self-Concept: Social Product and Social Force." Pp. 593-624 in *Social Psychology: Sociological Perspectives*, edited by Morris Rosenberg and Ralph H. Turner. New York: Basic.

Rosenberg, Morris, and Leonard Pearlin. 1978. "Social Class and Self-Esteem among Children and Adults." *American Journal of Sociology* 84:53-77.

Rosenberg, Morris, and Roberta G. Simmons. 1972. *Black and White Self-Esteem: The Urban School Child*. Rose Monograph series. Washington, D.C.: American Sociological Association.

Sampson, W., and V. Milam. 1975. "The Intraracial Attitudes of the Black Middle-Class: Have They Changed?" *Social Problems* 23:153-65.

Black Americans

Schwalbe, Michael L. 1985. "Autonomy in Work and Self-Esteem." *Sociological Quarterly* 26:519-35.

Shingles, Richard D. 1981. "Black Consciousness and Political Participation: The Missing Link." *American Political Science Review* 75:76-91.

Simmons, Roberta G. 1978. "Blacks and High Self-Esteem: A Puzzle." *Social Psychology* 41:54-57.

Simmons, Roberta G., Leslie Brown, Diane Mitsch Bush, and Dale A. Blyth. 1978. "Self-Esteem and Achievement of Black and White Adolescents." *Social Problems* 26:86-96.

Swann, William B., Jr. 1983. "Self-Verification: Bringing Social Reality into Harmony with the Self." Pp. 33-66 in *Psychological Perspectives on the Self*, vol. 2. Edited by Jerry Suls and Anthony G. Greenwald. Hillsdale, N.J.: Erlbaum.

Taylor, Marylee C., and Edward J. Walsh. 1979. "Explanations of Black Self-Esteem: Some Empirical Tests." *Social Psychology Quarterly* 42:242-53.

Toomer, J. 1975. "Beyond Being Black: Identification Alone Is Not Enough." *Journal of Negro Education* 44:184-99.

Wright, James D. 1976. *The Dissent of the Governed: Alienation and Democracy in America*. New York: Academic.

Wylie, Ruth. 1979. *The Self-Concept*, rev. ed. Vol. 2: *Theory and Research on Selected Topics*. Lincoln: University of Nebraska Press.

Zurcher, Louis A., Jr. 1977. *The Mutable Self: A Self-Concept for Social Change*. Beverly Hills, Calif.: Sage.

APPENDIX

TABLE A1

CORRELATIONS, MEANS, AND STANDARD DEVIATIONS

	1	2	3	4
1. Education	1.000	.586***	.350***	-.031
2. Occupational prestige	1.000	.347***	-.034
3. Personal income382***	...	1.000	-.082**
4. Likely lose job	1.000
5. Promotion chances (low)
6. Job performance	-.029078***	...
7. Job satisfaction
8. Employment status (not employed)	-.316***	...	-.406***	...
9. Family and friendship	-.128***	...	-.011	...
10. Religious involvement	-.122***	...	-.114***	...
11. System-blame019021	...
12. Interracial contact360***242***	...
13. Black identity	-.270***	...	-.154***	...
14. Black separatism	-.242***	...	-.114***	...
15. Gender (female)016	...	-.335***	...
16. Age	-.489***	...	-.054***	...
17. Racial self-esteem073**098***	...
18. Personal efficacy152***211***	...
19. Personal self-esteem065**119***	...
Mean	11.048	...	8.0 ^a 6.92	...
SD	3.344	...	7.3 ^a 6.65	...

TABLE A1 (*Continued*)

	12	13	14	15
1. Education302***	-.274***	-.212***	.070*
2. Occupational prestige254***	-.181***	-.160***	.072*
3. Personal income233***	-.158***	-.104***	-.313***
4. Likely lose job004	-.007	.025	-.045
5. Promotion chances (low)	-.067*	-.005	.033	.070*
6. Job performance	-.069*	.204***	.002	.027
7. Job satisfaction	-.055	.207***	-.007	.043
8. Employment status (not employed)
9. Family and friendship	-.125***	.328***	.097**	.056
10. Religious involvement	-.101**	.347***	.045	.195***
11. System-blame	-.023	-.022	.112***	-.039
12. Interracial contact	1.000	-.258***	-.216***	-.001
13. Black identity	-.237***	1.000	.259***	-.005
14. Black separatism	-.210***	.287***	1.000	-.060*
15. Gender (female)	-.051*	-.014	-.039	1.000
16. Age	-.286***	.288***	.161***	-.010
17. Racial self-esteem092***	.120***	.103***	-.033
18. Personal efficacy073**	.000	.013	-.098***
19. Personal self-esteem005	.141***	.051*	-.032
Mean	1.927	3.358	2.971	.617
SD720	.501	.714	.486

NOTE.—Data presented below the diagonal are for all respondents ($N = 1,609$); those above the diagonal are for currently employed respondents ($N = 847$). Missing data were deleted listwise.

* $P \leq .05$.

** $P \leq .01$.

*** $P \leq .001$.

5	6	7	8	9	10	11
-.150***	-.019	-.136***	...	-.123***	-.051	.057*
-.210***	.068*	.018	...	-.063*	-.010	.059*
-.149***	-.004	.013	...	-.035	-.078*	.064*
.102***	-.118***	-.206***	...	-.158***	-.067*	.036
1.000	-.105***	-.261***	...	-.086**	-.048	.065*
...	1.000	.220***455***	.136***	-.003
...	...	1.000261***	.150***	-.073*
...	-.088***	...	1.000
...373***017	1.000	.206***
...190***037	.266***	1.000
...	...	-.011021	-.011	-.002
...	...	-.083***	...	-.184***	-.148***	-.156***
...178***049*	.354***	.344***
...037082***	.114***	.067**
...	...	-.009156***	.049*	.208***
...185***215***	.264***	.372***
...046*	...	-.068**	.124***	.022
...097***	...	-.076***	.146***	.063**
...199***	...	-.141***	.326***	.214***
...	4.620405	-.009	.001	1.191
...	.640491	.980	.983	.810

16	17	18	19	Mean	SD
-.338***	.134***	.263***	.041	11.968	2.799
-.158***	.142***	.258***	.062*	.186	.995
.098**	.097**	.238***	.056	10,713.11	7,697.65
-.088**	-.076*	-.081**	-.104***	1.834	.939
.163***	-.020	-.097**	-.059*	2.582	1.161
.150***	.092**	.126***	.228***	4.649	.527
.252***	-.021	.005	.104***	3.168	.834
...
.221***	.099**	.084**	.292***	-.036	.946
.295***	.028	.032	.228***	-.033	.995
.047	.183***	.053	.024	1.175	.818
-.183***	.086**	.108***	-.018	2.025	.690
.313***	.112***	-.047	.137***	3.331	.483
.103***	.105***	-.022	.051	2.906	.680
.019	.015	-.054	-.002	.549	.498
1.000	.011	.051	.099**	38.615	13.747
.012	1.000	.189***	.178***	3.081	.435
.093***	.158***	1.000	.279***	4.076	2.555
.076***	.180***	.298***	1.000	21.496	2.317
41.973	3.055	3.935	21.177		
17.360	.442	2.557	2.534		

Long-Term Trends in Occupational Segregation by Sex¹

Jerry A. Jacobs
University of Pennsylvania

This paper reexamines long-term trends in occupational segregation by sex, using the double-coded 1900 and 1910 Public Use Samples. The analysis addresses the ambiguity in the measurement of long-term trends that arises from using inconsistent or highly aggregated occupational classifications. The revised measures indicate that occupational segregation by sex remained quite constant from 1900 through 1970, although segregation in nonfarm occupations declined slowly. Occupational sex segregation declined between 1970 and 1980 and continued to decline through 1986. An accurate assessment of historical trends is a necessary starting point for theoretical explanations of occupational sex segregation.

Occupational sex segregation has become an increasingly important area of multidisciplinary research. In 1985, over two-thirds of working women were employed in occupations that were 70% or more female.² About 25% of the gender gap in wages has been attributed to the sex segregation of occupations.³ Research interest in this area has been spurred by the rise in women's labor-force participation (Goldin 1983; Smith and Ward 1984), the women's movement (Klein 1984), and most recently by the public debate surrounding comparable-worth proposals (Remick 1984).

In 1968, Gross reported data that documented the remarkable stability of occupational segregation by sex since the turn of the century. This important finding set the stage for much subsequent research. The long-

¹ The comments of Nancy E. Durbin, Paula England, Edward Gross, Ann R. Miller, Samuel H. Preston, and anonymous reviewers on earlier drafts are greatly appreciated. Requests for reprints should be sent to Jerry A. Jacobs, Department of Sociology, University of Pennsylvania, Philadelphia, Pennsylvania 19104.

² Calculated from the 1986 Current Population Survey Annual Average data reported in U.S. Bureau of Labor Statistics (1987).

³ Sorensen (1987) reviews studies reporting that between 9% and 38% of the earnings gap is attributable to sex segregation (averaging 25%), and finds 27% in her own study when she controls for many individual characteristics as well as for industry. A U.S. Bureau of the Census report (1987) indicates that 17%–30% of the wage gap between men and women is due to women's concentration in female-dominated occupations.

Occupational Segregation

term stability of sex segregation is frequently the starting point for theoretical discussions in this area (Bielby and Baron 1986; Greenberger and Steinberg 1983; Oppenheimer 1970; Marini and Brinton 1984; Goldin 1983). This finding has raised serious questions for the human capital perspective in economics, as well as for the modernization approach in sociology (Polacheck 1979; Smelser 1959). Whereas both these views predicted increasing integration as women's labor-force participation increased, the overall trends appeared to show no such change. Two lines of reasoning in the human capital framework lead to a prediction of declining sex segregation. One, shared with the modernization perspective in sociology, holds that discrimination (ascriptive criteria) will be eroded by the competitive operation of the marketplace because discrimination is held to cost those who discriminate (Becker 1957). If a component of occupational segregation by sex is due to discrimination, the human capital model predicts its demise. But the human capital model also predicts the decline of voluntary segregation that may result from women's choices. Increasing wages lead more women into the labor force (Mincer 1962) for longer spells (Smith and Ward 1984), which in turn leads them to invest more heavily in labor-market skills (Polacheck 1979), which in turn should reduce the sexual differentiation of occupational choices. The alternative views, which emphasize the continuity of labor-market structures or the interest of men in preserving patriarchy, take the long-term stability of sex segregation as one of the important patterns to be explained (Strober 1984; Milkman 1987; Cohn 1985; Reskin 1988). While no single datum is decisive for the testing of different theories because, for apparently anomalous results, each theory can offer a variety of accounts, evidence on the long-term stability of sex segregation has constituted important grist for many theoretical mills.

Unfortunately, the inconsistency of occupational classifications from census to census has raised questions about the accuracy of this historical picture. This issue has been debated by students of sex segregation without an entirely satisfactory conclusion. In this article, I reexamine the question of long-term trends in occupational sex segregation with new data and assess the implication of my results for different theories of sex segregation.⁴

MEASURING LONG-TERM TRENDS

Gross (1968) analyzed changes in sex segregation for each decennial census from 1900 to 1960. He reported that in 1900 66.9% of employed

⁴ For a more extended analysis of these issues and related measures of segregation, see Jacobs (in press).

American Journal of Sociology

women would have had to change occupations for women to be distributed in the same manner as men. Gross found that this level remained at a nearly identical level (68.4%) in 1960. Thus, despite vast changes in the economy, the technology of production, and the mix of employment and despite the transition from a primarily agricultural society to a postindustrial society, Gross's data indicated that fundamental differences in work performed by men and women have persisted.

Williams (1979) criticized Gross for failing to standardize categories across censuses. Because each census employed a different system of coding occupations, Williams suggested that Gross's intercensal comparisons were likely to be flawed because of changes in occupational classifications. Williams maintained that increasingly detailed occupational categories in recent censuses tended to inflate the measures of segregation and consequently suppress the pattern of increasing integration. Williams aggregated occupations into larger, temporally consistent categories and found slow but consistent declines in sex segregation over time. England (1981), however, noted that Williams's approach exaggerates declines over time because it ignores real occupational differentiation that has taken place. Williams's reliance on aggregated categories ignores the emergence of new, segregated occupations, an especially important consideration over extended time periods. England maintained that while Gross's approach tends to underestimate change, Williams's tends to exaggerate change. She concluded that while there have been changes over the course of the century, the precise magnitude of these changes is hard to estimate. Most students of sex segregation have agreed that the changes during the century have been small, yet the evidence supporting this conclusion has not been entirely satisfactory.

Fortunately, data from the 1900 and 1910 censuses enable me to reexamine this conclusion. Data on samples of individuals in the manuscript censuses have been culled, computerized, and made available for research. The occupations of employed individuals in the 1900 sample have been double-coded, once with the 1900 occupational classification and once with the 1950 census system. The 1910 sample occupations have been double-coded with the 1910 and 1980 occupational categories. These computerized samples allow for a more detailed and accurate reclassification of the occupational data of early censuses than the published data permit. Whereas change in the occupational classification of published data inevitably involves imposing greater aggregation, the availability of the original survey responses allows for the creation of finer distinctions.

The double-coding strategy circumvents the problem of changing classifications. The effect of different occupational classifications can be estimated by calculating indices of segregation for 1900 using both the detailed 1900 and the detailed 1950 census categories. If one obtains es-

Occupational Segregation

sentially the same results by using the more recent, more detailed classification scheme as had been obtained with the contemporaneous occupational codes, then one can be reasonably sure that the results are not artifacts of the coding. I compare temporally distant years with the more recent coding system, providing comparability without sacrificing detail in measurement. I compare 1900 and 1950 by using the 1950 coding system for both years and compare 1910, 1970, 1980, and 1986 by using the 1980 coding system for each year. This procedure allows for the best possible assessment of long-term trends. It addresses Williams's concern by relying on the same categories for all years. It also addresses England's concern by using the most detailed and most recent categories: newly developed occupations simply appear empty in the earlier census.

The following examples of changing occupational classifications highlight the problem of comparability. In 1900 and 1910, barbers and hairdressers were combined in a single category, while in the 1980 classification system, these were distinct occupations. Similarly, in 1900 and 1910, elementary and high school teachers were combined in a single category, whereas in 1980, elementary and secondary teachers were reported separately. In both these examples, the combined category would appear more integrated by sex since the representation of women in one group (hairdressers and elementary teachers) is higher than in the other (barbers and high school teachers). Clearly, these are instances of change in the classification system that are not prompted by changes in the nature of work. One would prefer to compare the two time points with the same occupational classification system.

However, one should note that in other cases the use of an occupational classification system developed for a later period potentially poses a concern about the validity of the measure for the earlier time period. In the 1900 occupational classification system, "broom and brush makers," "corset makers," "straw workers," and "well borers" are listed as separate occupations. In subsequent censuses, these titles were lost as these jobs declined or were subsumed under other titles. As the nature of work changes over time, there may be no ideal way to compare occupational distributions at different time periods. Dropped titles occur mostly in manufacturing employment; there is greater continuity of titles among the professions and craft occupations.

Two points should be noted concerning the issue of the validity of classifications. First, this problem is more serious in knowing the nature of the work than it is in measuring sex segregation. My examination of the occupational titles phased out in later censuses does not suggest that I am significantly misrepresenting the sex composition of occupations in the earlier time period. Second, the use of a variety of classification systems for the same period minimizes this problem; if they all give the same

American Journal of Sociology

answer, we can be reasonably assured that the result is not an artifact of coding.⁵

A clarification of a related concern is in order. I do not claim that the nature of the work is the same in an identically titled occupation in 1910 and 1980. This continuity would be the exception, not the rule. The question I am attempting to answer is whether the sex composition of employment has been stable, for which purpose the data mentioned above will provide the best available answer.

A steep and consistent decline in sex segregation since the turn of the century would lend support to the human capital and modernization perspectives, which predict that the unfettered operation of the market economy undermines discrimination and promotes the ascendancy of universalistic criteria. In contrast, the long-term stability of sex segregation will support structuralist perspectives on sex segregation. Structuralists argue that sex segregation tends to be self-sustaining since, once institutionalized, sex-typed employment patterns are hard to eradicate (Roos and Reskin 1984). While these results will not constitute a decisive test between theories of sex segregation, this analysis is nonetheless essential in providing an accurate starting point for theoretical explanations.

DATA AND METHODS

As I noted above, in order to assess the effect of changing occupational classifications, I examine sex segregation in 1900 with both the 1900 and the 1950 census occupational classification systems. Similarly, I measure the degree of segregation in 1910 with both the 1910 and 1980 census occupational classification systems. In addition, I compare 1910 with 1970, 1980, and 1986, using the 1980 occupational codes in each case.

Data regarding the labor force in 1900 are obtained from the 1900 United States Census Public Use Sample. A sample of 100,438 individuals was drawn from the 1900 manuscript census, including 37,452 individuals with codable occupational information (30,584 men and 6,868 women). Details of the sampling design and occupational coding proce-

⁵ A related problem that has been suggested is that using the classification system for the later period would result in the imposition of artificial distinctions on the earlier period. This issue is of more theoretical than practical concern. Ann Miller, who directed the occupational coding for the 1910 project, indicates that this problem did not arise (personal communication). In any event, coders often do not have sufficient details to make artificial distinctions, even if they were called for. Indeed, the larger problem is that the data do not always allow the coder to make all the distinctions called for. However, this is true for contemporary censuses as well. The problem is handled the same way for contemporary and historical coding; when insufficient information is available to make a distinction between detailed occupations, the individual is placed in the appropriate "not elsewhere classified" category.

Occupational Segregation

dures are described in the *1900 Public Use Sample User's Handbook* (Graham 1980). Data for 1910 are obtained from the 1910 Public Use Sample, compiled by a research team at the University of Pennsylvania (Strong 1988). The sample of 366,239 individuals from the 1910 manuscript census includes 120,738 men and 32,886 women with codable occupations. Data for 1980 are obtained from the 1980 census (U.S. Bureau of the Census 1984). A sample from the 1970 census was assigned 1980 occupational codes to facilitate historical comparisons. The data were published as part of the 1980 census volume. And 1986 data are obtained from annual average Current Population Survey data (U.S. Bureau of Labor Statistics 1987).

Special attention needs to be paid to several characteristics of the 1900 labor force. One important consideration in examining occupational segregation at the turn of the century is the size of the agricultural sector. In 1900, 37.5% of the labor force was employed in the agricultural sector, a figure that declined to 2.9% by 1980.

There are several reasons one might choose to exclude agriculture from an analysis of long-term trends. First, there may be more measurement error in the classification of men and women in farm work. The extent of farm women's labor-force participation has been a matter of debate among economic historians for some time (Oppenheimer 1970). Further, occupational distinctions between farm men and women may be artificial since census coders may classify women as unpaid family workers, while men are classified as farmers, no matter how the work is actually divided. Perhaps more important, census takers group most farm employment into a few broad categories, which tends to reduce the measured level of segregation.⁶

Second, theories about trends in occupational segregation apply most specifically to competition in industrial society and may not apply with equal force to the declining agricultural sector. The degree to which competitive forces drive employers to find the lowest-wage worker for a given job applies more to the industrial economy because the contribution of family members to farm work is often outside the cash nexus of the broader economy.⁷ The degree of sex segregation on family farms is as

⁶ The coding rule in 1900 was to allocate wives, sons, and daughters of farmers to the category "farm laborers, family workers." Women could be classified as "farm owners" if they were heads of household, or as "farm laborers, wage workers" if they were unrelated to the farm owner. In the 1900 Public Use Sample, 440 women were listed as farm owners, managers, or foremen (vs. 7,169 men); 498 were listed as family workers (vs. 2,458 men); and 383 were listed as farm laborers, wage workers (vs. 2,571 men). The distinctions, of course, do little to clarify the actual sexual division of labor on farms.

⁷ This is not to deny the competitive forces working in agriculture. But one might argue that the force of tradition is stronger in agriculture.

American Journal of Sociology

much a matter of the division of household labor as it is an indicator of the degree of occupational sex segregation (Rosenfeld 1985).

Third, even if farms were theoretically interesting in this context, their very size and rapid decline would raise the possibility of testing a compositional effect on long-term trends. If nonfarm sex segregation were to decline over time, then we could conclude that this decline in sex segregation was masked by the rapid decline of the relatively highly integrated farm sector.

A second feature of the 1900 labor force that should be noted is the presence of child labor. In 1900, child labor remained a part of the American labor force, with occupational statistics reported for individuals aged 10 and above. An analysis of subsequent periods must adjust for the decline in child labor. While I present results that include children and agricultural workers, I also report figures for individuals above the age of 16 in nonfarm occupations.

Most research on occupational segregation by sex has employed one measure of segregation, the index of dissimilarity (D) (for other approaches, see Massey and Denton 1989; and Jacobs 1985, 1986, in press). This measure taps the main dimension of segregation, namely, the degree to which two groups are unevenly distributed over a set of categories. The index of dissimilarity has a convenient interpretation: it represents the proportion of women who would have to change occupations in order to be distributed in the same manner as men. The measure D is symmetrical; that is, the same proportion of men would have to change occupations to be distributed in the same manner as women.⁸

When trends are examined over time, the index of dissimilarity is often supplemented with a size-standardized index of dissimilarity. This index indicates what the degree of segregation would be if all occupations were of equal size. By holding the mix of occupations constant, this measure indicates what changes over time would have occurred if there had been no change in the relative sizes of the occupations.⁹

⁸ The formula for D is:

$$D = \frac{1}{2} \sum_{i=1}^n |(W_i/W) - (M_i/M)| \times 100, \quad (1)$$

where W_i is the number of women in occupation i , W is the total number of women, M_i is the number of men in occupation i , M is the total number of men, and n is the number of occupations. The measure D varies from 0 to 100, with 0 representing a perfectly even distribution and 100 representing perfect segregation.

⁹ The formula for the size-standardized index of dissimilarity is:

$$D_{\text{standardized}} = \frac{1}{2} \sum_{i=1}^n |[(W_i/T_i)/\Sigma(W_i/T_i)] - [(M_i/T_i)/\Sigma(M_i/T_i)]| \times 100, \quad (2)$$

Occupational Segregation

RESULTS: SEX SEGREGATION IN THE UNITED STATES SINCE 1900

Table 1 reports comparisons of occupational segregation by sex for 1900 and 1950. For 1900, results using both the 1950 occupational classification and the 1900 classification are reported. In addition, table 1 compares results that include farm and child labor with those that exclude these groups.

Gross (1968) reported that the level of occupational segregation (D) between men and women in 1900 was 66.9. The comparable figure for the 1900 Public Use Sample, when the 1900 coding scheme is used, is 64.9. The difference between these figures may be due to sampling variability and may also reflect small differences in the way the 1900 classification rules were applied by the Public Use Sample coders. When one employs the 1950 coding scheme, the level of sex segregation is 65.0. The difference between the 1900 and 1950 coding schemes in the portrayal of sex segregation in 1900 is a trivial one-tenth of one percent, 64.9 versus 65.0. The 1950 figure was 67.3, indicating virtually no change over this long period.¹⁰ Thus, the historical trends Gross reported were almost exactly correct despite the occupational classification problem. This analysis indicates that available detailed and comparable occupational categories confirm the long-term stability documented by Gross.

As indicated above, long-term comparisons should properly focus on the nonfarm labor force above the age of 16. When the agricultural sector and child labor are removed from the analysis, the results indicate that occupational segregation by sex has in fact been declining slowly. In 1900, D was 75.9 for individuals in nonfarm occupations, a figure that declined to 66.9 in 1950 for the nonfarm occupations. While Gross's estimate of no significant change through 1950 is correct, the analysis of nonfarm occupations gives a somewhat more optimistic picture of the long-term trends in this area.

Gross reported that the size-standardized measure of segregation declines gradually over time. The size-standardized index of dissimilarity declined from 78.5 in 1900 to 61.3 in 1950. This statistic indicates that the more integrated occupations declined in size in this period relative to the more segregated occupations. In other words, had the size distribution of occupations in 1900 persisted, sex segregation would have declined by 1950. That trends in the sizes of occupations inhibited the decline in sex segregation is clearly evident from these results and from those obtained by Gross.

where T_i is the total number of men and women in occupation i , and other terms are defined as in n. 8 above.

¹⁰ My figure corresponds with the figure Williams (1979) reports for 1950, which is just slightly higher than that reported by Gross (1968).

TABLE 1
INDICES OF DISSIMILARITY, 1900 AND 1950

YEAR	DATA	Occupational Coding System	ENTIRE LABOR FORCE		NONFARM LABOR FORCE OVER AGE 16	
			D	D standardised	D	D standardised
1900	Published data*	1900	66.9	70.3
1900	Public Use Sample	1900	65.0	75.1	75.3	76.3
1900	Public Use Sample	1950	64.9	78.5	75.9	76.1
1950	Published data†	1950	67.3	61.3	66.9	60.4

* Figures obtained from England (1981).

† U.S. Bureau of the Census (1953, table 124).

Occupational Segregation

The present results suggest that much of this compositional effect is due to the fact that one very large and declining occupational group, agriculture, had a relatively low level of segregation in 1900. The decline in the unstandardized index of dissimilarity obtained when farming is excluded from the analysis is about half as large (9 points, from 75.9 to 66.9) as that obtained in a size-standardized analysis of changes over time (17.2 points, from 78.5 to 61.3). (Results for the size-standardized measure for the nonfarm occupations are quite similar to those for the entire labor force. This is true because the removal of a few occupations makes little difference when each occupation is accorded the same weight.)

It should also be noted that the level of sex segregation was quite low among children in the labor force. In 1900, the index of dissimilarity among those under age 16 was 44.0, much lower than that obtained for the labor force as a whole, and perhaps lower than among present-day teenagers (Greenberger and Steinberg 1983).¹¹ The higher level of segregation found for the nonfarm labor force over the age of 16, then, is partly due to the removal of the relatively less segregated child laborers.

Table 2 presents results for 1910 using three different types of data: published 1910 census data using 1910 occupational codes; 1910 Public Use Sample data using 1910 occupational codes; and 1910 Public Use data using 1980 codes. There is a slightly greater range of results for 1910 than for 1900 (63.0–69.0), but the substantive conclusion that approximately two-thirds of men and women working in 1910 would have had to change occupations to be evenly distributed is corroborated by each of these measures. Further, these measures are all quite close to those obtained for 1900 with two different occupational classification systems.

Table 2 also brings trends in sex segregation up to date, using 1980 occupational codes to compare 1910, 1970, 1980, and 1986. Between 1910 and 1970, occupational segregation by sex remained constant. However, men and women in nonfarm occupations did experience a decline in sex segregation, with the index of dissimilarity declining from 74.3 in 1910 to 67.5 in 1970 to 59.7 in 1980. Sex segregation in nonfarm occupations declined by roughly one point per decade, on average, between 1910 and 1970.

The nonfarm trends moved in the same direction as the size-standardized trends. The size-standardized index of dissimilarity declined from 73.8 in 1910 to 61.3 in 1970, a drop of 12.5 points. The drop in the size-standardized measure is roughly twice as large as that in the nonfarm labor force (6.8 points, from 74.3 to 67.5), as was also the case in the 1900 and 1950 comparison. The long-term decline in agricultural employment

¹¹ They report high levels of sex segregation among teenagers, but do not report a specific index of dissimilarity.

TABLE 2
INDICES OF DISSIMILARITY, 1910, 1970, AND 1980: 1980 OCCUPATIONAL CLASSIFICATION SYSTEM

YEAR	DATA	OCCUPATIONAL CODING SYSTEM	ENTIRE LABOR FORCE		NONFARM LABOR FORCE OVER AGE 16	
			D	D _{standardized}	D	D _{standardized}
			1910	69.0	68.1	...
1910	Published data*	1910	67.0	70.5	74.9	71.4
1910	Public Use Sample	1910	63.0	73.8	74.3	73.7
1910	Public Use Sample	1980	67.6	61.3	67.5	61.2
1970	Published data†	1980	59.8	55.3	59.7	55.3
1980	Published data†	1980	57.3	56.5	57.2	56.6
1986	Published data‡					

* Figures obtained from England (1981).

† U.S. Bureau of the Census (1984, table 323).

‡ U.S. Bureau of Labor Statistics (January 1987).

Occupational Segregation

inhibited declines in overall sex segregation and, by itself, represents a significant component of the compositional effect documented with the size-standardized measure.

The gradual decline in sex segregation that began in the 1970s continued into the 1980s, with the index of dissimilarity dropping to 57.3 by 1986. This decline thus represents a continuing trend and is not a temporary, artifactual phenomenon caused by a limited set of occupations moving from male-dominated to female-dominated fields (Reskin and Roos, *in press*).

DISCUSSION

Four different occupational classification systems are employed in this analysis: 1900 and 1950 occupational coding systems are used for measuring segregation in 1900, and 1910 and 1980 systems are used for measuring 1910. All four classification systems produce approximately the same levels of sex segregation for the 1900–1910 period, ranging from a low of 63.0 to a high of 69.0. There can be little doubt about the level of occupational sex segregation at the turn of the century or its continuation for most of the century. Of course, the levels of segregation at the firm or job level cannot be addressed by these data.

My reassessment of occupational trends since the turn of the century reinforces conventional wisdom in some ways and modifies it in others. Gross's (1968) conclusions about the long-term stability in sex segregation are corroborated and are not artifacts of changing classification. However, nonfarm segregation has declined somewhat since 1900. Nonfarm segregation was about 10 points higher than overall segregation in 1910. There was approximately a seven-point decline in segregation between 1910 and 1970 and another seven-point decline since 1970. In other words, there was roughly as much change in nonfarm sex segregation between 1910 and 1970 as there was between 1970 and 1980.

These results do not constitute definite support for any theory of sex segregation. Human capital theorists might point to the recent changes as evidence that the market is responsive to changing investments of women, while structuralists might point to the continuity through 1970 as evidence of the persistence of discriminatory barriers against women.

The present results seem consistent with Kessler-Harris's (1982) view of change in women's working conditions over time. She argues that over the long term, change has been incremental but concludes that there are potentially radical consequences of incremental change. In short, the glacial pace of change in sex segregation through 1970 has given way to modest but consistent change since that time. While the emphasis on continuity has been an important strength, the structuralist perspective

American Journal of Sociology

should attempt to identify conditions under which the degree of sex segregation would be expected to change (Baron, Mittman, and Newman 1988).

REFERENCES

Baron, James N., Brian S. Mittman, and Andrew E. Newman. 1988. "Organizational and Environmental Sources of Change in Sex Segregation within the California Civil Service." Paper presented at the American Sociological Association Meetings, Atlanta.

Becker, Gary. 1957. *The Economics of Discrimination*. Chicago: University of Chicago Press.

Bielby, William T., and James N. Baron. 1986. "Men and Women at Work: Sex Segregation and Statistical Discrimination." *American Journal of Sociology* 91 (4): 759–99.

Cohn, Samuel. 1985. *The Process of Occupational Sex Typing*. Philadelphia: Temple University Press.

England, Paula. 1981. "Assessing Trends in Occupational Sex-Segregation, 1900–1976." Pp. 273–294 in *Sociological Perspectives on Labor Markets*, edited by Ivar Berg. New York: Academic.

Goldin, Claudia. 1983. "Historians' Consensus on the Economic Role of Women in American History: A Review Essay." *Historical Methods* 16 (2): 74–81.

Graham, Stephen N. 1980. *1900 Public Use Sample User's Handbook*. Seattle: Center for Studies in Demography and Ecology.

Greenberger, Ellen, and Laurence D. Steinberg. 1983. "Sex Differences in Early Labor Force Experience: Harbinger of Things to Come?" *Social Forces* 62 (2): 467–86.

Gross, Edward. 1968. "Plus ça change . . . ? The Sexual Structure of Occupations over Time." *Social Problems* 16:198–208.

Jacobs, Jerry A. 1985. "Sex Segregation in American Higher Education, 1948–1980." Pp. 191–214 in *Women and Work: An Annual Review*, edited by Laurie Larwood, Ann H. Stromberg, and Barbara Gutek. Beverly Hills, Calif.: Sage.

—. 1986. "Trends in Contact between Men and Women at Work, 1971–1981." *Sociology and Social Research* 70 (3): 202–6.

—. In press. *Revolving Doors: Sex Segregation and Women's Careers*. Stanford: Stanford University Press.

Kessler-Harris, Alice. 1982. *Out to Work: A History of Wage-earning Women in the United States*. New York: Oxford University Press.

Klein, Ethel. 1984. *Gender Politics: From Consciousness to Mass Politics*. Cambridge: Harvard University Press.

Marini, Margaret M., and Mary C. Brinton. 1984. "Sex Typing in Occupational Socialization." Pp. 192–232 in *Sex Segregation in the Workplace: Trends, Explanations, Remedies*, edited by Barbara Reskin. Washington, D.C.: National Academy of Sciences.

Massey, Douglas S., and Nancy A. Denton. 1989. "The Dimensions of Residential Segregation." *Social Forces* 67 (2): 281–315.

Milkman, Ruth. 1987. *Gender at Work: The Dynamics of Job Segregation by Sex during World War II*. Urbana: University of Illinois Press.

Mincer, Jacob. 1962. "Labor Force Participation of Married Women: A Study of Labor Supply." Pp. 63–97 in *Aspects of Labor Economics*, edited by H. Gregg Lewis. Princeton: Princeton University Press.

Occupational Segregation

Oppenheimer, Valerie Kincade. 1970. *The Female Labor Force in the United States*. Westport, Conn.: Greenwood.

Polacheck, Solomon W. 1979. "Occupational Segregation among Women: Theory, Evidence and a Prognosis." Pp. 137-57 in *Women in the Labor Market*, edited by Cynthia B. Lloyd et al. New York: Columbia University Press.

Remick, Helen, ed. 1984. *Comparable Worth and Wage Discrimination: Technical Possibilities and Political Realities*. Philadelphia: Temple University Press.

Reskin, Barbara F. 1988. "Bringing the Men Back In: Sex Differentiation and the Devaluation of Women's Work." *Gender and Society* 2 (1): 58-81.

Reskin, Barbara F., and Patricia A. Roos. In press. *Gendered Work and Occupational Change*. Philadelphia: Temple University Press.

Roos, Patricia, and Barbara Reskin. 1984. "Institutional Factors Contributing to Sex Segregation in the Workplace." Pp. 235-60 in *Sex Segregation in the Workplace: Trends, Explanations, Remedies*, edited by Barbara Reskin. Washington, D.C.: National Academy of Sciences.

Rosenfeld, Rachel. 1985. *Farm Women: Work, Farm and Family in the United States*. Chapel Hill: University of North Carolina Press.

Smelser, Neil J. 1959. *Social Change in the Industrial Revolution*. Chicago: University of Chicago Press.

Smith, James P., and Michael P. Ward. 1984. *Women's Wages and Work in the Twentieth Century*. Santa Monica, Calif.: Rand.

Sorensen, Elaine. 1987. "The Wage Effects of Occupational Sex Composition: A Review and New Findings." Paper presented at the Rutgers Colloquium on Comparable Worth, New Brunswick, N.J.

Strober, Myra H. 1984. "Toward a General Theory of Occupational Sex Segregation: The Case of Public School Teaching." Pp. 144-56 in *Sex Segregation in the Workplace: Trends, Explanations, Remedies*, edited by Barbara Reskin. Washington, D.C.: National Academy of Sciences.

Strong, Michael. 1988. *1910 Public Use Sample Handbook*. Philadelphia: University of Pennsylvania, Population Studies Center.

U.S. Bureau of the Census. 1953. *U.S. Census of Population, 1950*. Vol. 2: *Characteristics of the Population*. Washington, D.C.: Government Printing Office.

—. 1984. "Detailed Population Characteristics." In *1980 Census of Population*, vol. 1. Washington, D.C.: Government Printing Office.

—. 1987. "Male-Female Differences in Work Experience, Occupation, and Earnings: 1984." *Current Population Report*, ser. P-70, no. 10. Washington, D.C.: Government Printing Office.

U.S. Bureau of Labor Statistics. 1987. *Employment and Earnings*. Washington, D.C.: Government Printing Office.

Williams, Gregory. 1979. "The Changing U.S. Labor Force and Occupational Differentiation by Sex." *Demography* 16:73-88.

Commentary and Debate

To conserve space for the publication of original contributions to scholarship, the comments in this section must be limited to brief critiques. They are expected to address specific errors or flaws in articles and reviews published in the *AJS*. Comments on articles are not to exceed 1,500 words, those on reviews 750 words. Longer or less narrowly focused critiques should be submitted as articles. Authors of articles and reviews are invited to reply to comments, keeping their replies to the length of the specific comment. The *AJS* does not publish commenters' rebuttals to authors' replies. We reserve the right to reject inappropriate or excessively minor comments.

REGION AND VIOLENT ATTITUDES RECONSIDERED: COMMENT ON DIXON AND LIZOTTE¹

In a recent article, Jo Dixon and Alan J. Lizotte (*AJS* 93 [September 1987]: 383–405) report an unsuccessful attempt to document the existence of a southern subculture of violence and its relation to gun ownership. Their article marks an important advance over previous studies of the southern subculture of violence, which fail to measure violent values independently of region (for a recent discussion, see McCall, Land, and Cohen [1989]). Because Dixon and Lizotte offer perhaps the first analytical design capable of specifying the links among region, violent attitudes, and individual behavior, their work merits careful scrutiny.

However, their work may be important for another reason as well. Despite this methodological sophistication, their conceptualization of the southern subculture of violence may be flawed. Careful consideration of recent discussions of the southern subculture-of-violence thesis suggests that, instead of disconfirming its core contentions, the research of Dixon and Lizotte actually provides this theory with its strongest empirical support to date.

¹ We would like to thank John S. Reed and Kenneth C. Land for their comments on an earlier draft.

Permission to reprint a comment printed in this section may be obtained only from the author.

Commentary and Debate

In brief, Dixon and Lizotte proceed as follows. They perform factor analysis on a set of items from General Social Surveys (GSS) that gauges the acceptability of violent behavior in various hypothetical situations (see Smith 1981). They find two orthogonal factors that they label "violent" and "defensive" attitudes (see p. 394 for a list of the items that comprise these respective factors). The remainder of their analysis hinges on (1) documenting that individuals socialized in the South or individuals residing in the South at the time of the interview are especially likely to hold the "violent" attitudes and (2) demonstrating, in turn, that these "violent" attitudes significantly predict individual gun ownership.

Thus, theirs is a sound test of the southern subculture-of-violence thesis *only if* the survey items comprising the "violent" factor accurately represent the thrust of theoretical literature on southern violence. However, we argue that the attitudes that they identify as "violent" do not reflect current thinking about southern culture. Moreover, we find that those values termed "defensive" actually permit a more accurate and effective test of the theory.

The reasoning employed by Dixon and Lizotte in labeling these factors is pivotal. Their key criteria for distinguishing violent and defensive attitudes are ad hoc and empirical, rather than theoretical. The authors note that the items that comprise the "violent" attitudinal factor "imply support for the use of *aggression in situations that provide little or no justification for violence*" (p. 390, emphasis added). Appealing to the common sense of the reader, the authors contend that such items would be more fundamental to violent cultures than would those of the "defensive" factor, which sanction violence in defense of women, children, and property.

Further, they claim that support for the "defensive" items is widespread in the sample. They reason, therefore, that defensive attitudes seem a part of mainstream American culture, in contrast to the more marginal "violent" orientations. Moreover, they cite the orthogonality of the factors to buttress their contention that violent and defensive attitudes are fundamentally different and unrelated. Finally, they account for this orthogonality by suggesting that "members of a subculture of violence might engage in abusive rather than defensive behavior with regard to women and children" (p. 391; see also p. 395).

We feel that a more appropriate strategy would make fuller use of the rich literature on the historical and contemporary development of a southern subculture of violence. As Wolfgang and Ferracuti (1967, pp. 150–61) point out, the content of the violent subcultures is culturally specific. Therefore, it is especially important to consider the "fit" between the hypothetical *incidents* of interpersonal violence identified in the GSS and the understanding of what is distinctive about violence. Otherwise, it

American Journal of Sociology

is difficult to claim that one has tested the "southern subculture-of-violence" thesis *per se*.

While a number of analysts have discussed distinctive aspects of southern culture and violence (see Brearley 1935; Cash 1941; Hackney 1969; Bruce 1979), perhaps the foremost contemporary proponent of the southern subculture-of-violence thesis is John Shelton Reed (1982). In an argument that parallels that of Wolfgang and Ferracuti (1967), Reed maintains that southerners are more prone to violence than other Americans, not in all situations but only in certain circumstances in which southern culture permits or demands violent response.

Reed's (1982) discussion is especially useful because one main intent of his writing is to distinguish *southern* violence from the aggressive and often random forms of violence feared and abhorred by most Americans (Reed 1982, p. 143). He presents evidence that much southern violence is "violence for cause." Two anecdotes are especially revealing in this regard. First, Reed tells of a southern newspaper editor threatened by an irate local citizen who disagreed with an editorial. On receiving the telephoned death threat, the editor opted to "oil his gun and wait" rather than flee or call for outside help. In another anecdote, an elderly southern man endured months of verbal harassment and pranks from young locals before shooting and injuring his tormentors. A jury of his peers declined to convict him, with one juror noting that the accused "wouldn't have been much of a man" had he permitted the continued abuse to go unpunished.

Reed uses these incidents and other selected artifacts of southern popular culture to argue that "sometimes . . . [southerners] are violent, even when they don't want to be, because there will be penalties (disgrace is a very effective one) for not being violent" (1982, p. 147). In sum, Reed seems to suggest that when confronted with unwarranted aggression or malicious behavior, interpersonal threats, and intentional affronts to honor, southerners may be culturally disposed to choose violent responses over whatever alternatives may exist.

This characterization of a southern subculture of violence recalls earlier discussions such as the one by Brearley (1935). He highlights residual aspects of "the feudal spirit" of the Old South, manifested in the salience of honor and loyalty. In Brearley's view, this accounts for several specific types of violence common among southerners: violence in cases of gross breaches of trust, injuries to family, and threats to the "good name" of women.

Such characterizations of the southern subculture of violence differ strikingly from the measures of "violent" attitudes employed by Dixon and Lizotte. For instance, one finds little in the literature on southern violence to suggest that southern norms encourage attacks on protesters

Commentary and Debate

exercising their constitutional rights. Indeed, it is not clear from previous studies why southerners would be exceptionally likely to approve *any* of the more aggressive types of interpersonal violence that figure in the indicator of "violent" attitudes used by Dixon and Lizotte. And there is certainly scant evidence of a regional culture that normatively embraces physical abuse of women and children (p. 395). These hypotheses may reflect unflattering ethnoregional stereotypes of southerners rather than theoretically grounded analyses.

There is, however, reason to suspect that southerners would readily sanction violence in defense of women, children, and property. It is these forms of violence—labeled "defensive" by Dixon and Lizotte—that are highlighted in most recent discussions of the southern subculture of violence. If *these*, rather than the "violent" attitudes, permit a more appropriate test of the theory, then we might expect (1) that southern socialization or residence is positively associated with *defensive* attitudes, and (2) that *defensive* attitudes are in turn significantly related to the ownership of firearms. And this is precisely what Dixon and Lizotte find.

First, despite their earlier claim that defensive attitudes are distributed widely throughout the sample, Dixon and Lizotte find that persons socialized in the South and currently residing there are significantly more likely to hold "defensive" attitudes than individuals socialized in other regions and currently residing outside the South. Second, although other differences with regard to region of socialization and region of residence are not significant, the direction of the coefficients suggests that *any* exposure to the South increases the tendency toward sanctioning defensive violence (p. 399). Third, defensive attitudes are significant predictors of gun ownership, net of the effects of a host of structural and demographic variables. Fourth, as one might have predicted from previous studies (see Erlanger 1976, p. 490), Dixon and Lizotte do not find similar relations with regard to their set of violent attitudes.

It is at this point that Dixon and Lizotte fall victim to their own errant labels. They argue, in light of the orthogonality of the violent and defensive attitudinal factors, that gun ownership centering on defensiveness is "unrelated to membership in a subculture of violence" (p. 400), and they conclude that their results "cast considerable doubt on the southern subculture-of-violence thesis" (p. 401). Indeed, for their purposes, the main theoretical lesson presented by their model of defensive attitudes is the confirmation that violent attitudes and defensive attitudes are diverse constructs (p. 399).

We have suggested here that these findings actually support a very different interpretation. Albeit unwittingly, Dixon and Lizotte have made a valuable contribution to the literature on gun ownership—and regional patterns of violence in general—by using attitudinal data from a national

American Journal of Sociology

sample to confirm the core arguments of the southern subculture-of-violence thesis that were previously advanced by Reed and others in largely anecdotal form. Subsequent research should focus on the following areas: (1) further specifying the situational character of southern attitudes toward violence and their links with actual violent behavior, (2) identifying the concrete social and institutional mechanisms through which such distinctive regional norms are sustained and transmitted, and (3) investigating the possible temporal declines in the distinctive southern attitudes regarding the use of violence that were forecast by previous analysts of southern violence (Bearley 1935, pp. 691-92).

CHRISTOPHER G. ELLISON

Duke University

PATRICIA L. McCALL

North Carolina State University

REFERENCES

Bearley, Huntington C. 1935. "The Pattern of Violence." Pp. 678-92 in *Culture in the South*, edited by W. T. Couch. Chapel Hill: University of North Carolina Press.

Bruce, Dickson D., Jr. 1979. *Violence and Culture of the Antebellum South*. Austin: University of Texas Press.

Cash, Wilbur J. 1941. *The Mind of the South*. New York: Knopf.

Erlanger, Howard S. 1976. "Is There a 'Subculture of Violence' in the South?" *Journal of Criminal Law and Criminology* 66:483-90.

Hackney, Sheldon. 1969. "Southern Violence." *American Historical Review* 74:906-25.

McCall, Patricia L., Kenneth C. Land, and Lawrence E. Cohen. 1989. "Violent Criminal Behavior: Is There a Continuing Influence of the South?" Working Paper. Duke University and University of California at Davis.

Reed, John Shelton. 1982. *One South: An Ethnic Approach to Regional Culture*. Baton Rouge: Louisiana State University Press.

Smith, Tom W. 1981. "Qualifications in Generalized Absolutes: Approval of Hitting Questions in the GSS." *Public Opinion Quarterly* 45:225-30.

Wolfgang, Marvin E., and Franco Ferracuti. 1967. *The Subculture of Violence*. London: Tavistock.

ON CULTURAL EXPLANATIONS OF SOUTHERN HOMICIDE: COMMENT ON DIXON AND LIZOTTE¹

Dixon and Lizotte's article, "Gun Ownership and the 'Southern Subculture of Violence,'" (*AJS* 93 [September 1987]: 383-405) adds another chapter to the debate about cultural explanations of southern violence. In

¹ We thank Hugh Whitt for his helpful critique of an earlier draft of this comment.

Commentary and Debate

brief, the authors evaluate Reed's (1972) hypothesis that southern culture has a positive effect on gun ownership by testing a probit model relating social and demographic characteristics, measures of violent attitudes, and socialization experiences to the possession of firearms by private citizens. Although they provide no analyses of lethal violence, Dixon and Lizotte (p. 402) assert that their "results indicate that the southern subculture-of-violence thesis is inadequate for explaining high rates of gun ownership *and* homicide in the South" (our italics). Considering the complex issues involved in research on southern violence (Huff-Corzone, Corzone, and Moore 1986), this statement is surprising.

Following Wolfgang and Ferracuti (1967), Dixon and Lizotte's conceptualization and measurement of subculture-of-violence membership are based on a narrow view that is inconsistent with the positions of researchers who attribute high rates of southern homicide to regional differences in culture. Furthermore, their finding that defensive attitudes are related to birth and residence in the South (table 3, p. 397) supports the proposition that regional differences in homicide are rooted in an exaggerated defensiveness among white southerners, a position asserted by researchers they criticize (e.g., Hackney 1969; Reed 1972).

Culture and Violence

Consistent with Wolfgang and Ferracuti (1967), Dixon and Lizotte adopt a view of subcultures of violence that emphasizes normative support for violence in interpersonal relations. From this perspective, persons socialized within a subculture of violence engage in violent acts because they hold "violent values" and "violent attitudes," that is, they approve of violence. Moreover, Dixon and Lizotte believe that such individuals are likely to express violence in a wide range of situations (pp. 391, 395). They measure membership in a subculture of violence through a "violent-attitudes scale" that is, in their words, "indicative of the set of violent values Wolfgang and Ferracuti (1967) deem the defining characteristic of subcultures of violence" (p. 396).

Dixon and Lizotte assume that Wolfgang and Ferracuti's subculture-of-violence theory has provided the theoretical foundation for research by Hackney (1969), Gastil (1971), and Reed (1972) that supports cultural explanations for high levels of gun ownership and homicide in the South (p. 388). However, support for this contention is lacking. In fact, important differences between Wolfgang and Ferracuti's thesis and the ideas advanced by these investigators are recognized in the literature (e.g., Erlanger 1976). We will briefly review the more important points of disagreement.

Hackney's (1969) article, "Southern Violence," stimulated current in-

American Journal of Sociology

terest in the traditionally high rates of homicide in the southern states. He does not reference Wolfgang and Ferracuti, nor does he use the term “subculture of violence.” Also, his attention is not restricted to interpersonal violence but, instead, focuses on the regional juxtaposition of high murder and low suicide rates, which he calls “a distinctly southern pattern of violence” (p. 908). He tentatively concludes that this “pattern of violence” stems from a worldview that defines the physical and social environment as threatening and hostile (p. 925). This conceptualization of cultural influence on violence is substantially broader than the idea of a regional tendency to approve of violence advanced by Dixon and Lizotte.

Gastil (1971) explicitly notes differences between his formulation of a “regional culture of violence” and the subculture-of-violence thesis associated with Wolfgang and Ferracuti (p. 416). Most important, Gastil explicitly rejects the notion that culture influences homicide levels only by providing approval for violent acts: “A violent tradition may be one that in a wide range of situations condones lethal violence, *or* it may be a tradition that more indirectly raises the murder rate” (p. 416).

Reed’s (1972) early work is consistent with Wolfgang and Ferracuti in some ways, but his later views diverge from their position (Reed 1982). For Reed, culture affects homicide by defining a range of situations in which violence is either accepted or required: “Southerners will not be more violent in *all* circumstances, but only in those where the culture permits or demands a violent response” (1982, p. 143). If violence is a normative response to particular situations, it is unnecessary to assume that “violent attitudes” and “violent values” are the only cultural elements that lead to violent acts: “Sometimes people are violent because they want to be and there is nothing to stop them. But sometimes people are violent, even when they *don’t* want to be, because there will be penalties (disgrace is a very effective one) for *not* being violent” (Reed 1982, p. 147).

The Violent-Attitudes Scale

Dixon and Lizotte measure membership in a subculture of violence by scores on a “violent-attitudes scale” constructed from answers to four questions included in the General Social Surveys (GSS) for 1976, 1980, and 1984 (table 2, p. 394). Each item asks if the respondent would approve of an adult male “striking” or “punching” another man who has committed a minor provocation. Two major problems with Dixon and Lizotte’s use of this measurement to test the ideas advanced by Hackney, Gastil, and Reed are apparent. First, the violent-attitudes scale is designed to test Wolfgang and Ferracuti’s subculture-of-violence thesis, but

their conceptualization of the relation between culture and violence is different than the one that has guided research on the South. Second, studies of southern violence have focused on homicide, and it is debatable if those who approve of low-level violence also accept acts that are more likely to result in serious injury or death.²

It is not surprising that Dixon and Lizotte report no significant relation between the violent-attitudes scale and birth or residence in the South. Whether this finding has any implication for the adequacy of cultural theories of southern homicide is doubtful.

The Defensive-Attitudes Scale

Dixon and Lizotte develop a second index measuring defensive attitudes from three items on the GSS. These questions ask respondents whether they would approve of a man "punching" an adult male stranger who, respectively, (1) "had hit the man's child," (2) "was beating up on a woman," and (3) "had broken into the man's house" (table 2). Dixon and Lizotte's (p. 391) assumption that the two indexes measure "fundamentally different attitudes" is perplexing. It is difficult to see how either scale can be interpreted as measuring an attitudinal dimension other than approval of low-level violence. Instead, the scales appear to measure "fundamentally different situations" (our quotation marks) in which violence may be used. A careful reading of the questions suggests that the violent-attitudes scale measures support for violent responses to nonphysical threats, while the defensive-attitudes scale taps approval for violence undertaken to protect persons from an assailant.³

Dixon and Lizotte report that the defensive-attitudes scale is positively related to both exposure to the southern region (table 3) and the likelihood of owning a gun (table 4). Consistent with their interpretation of Wolfgang and Ferracuti, they do not view this finding as supporting a cultural interpretation of southern violence. We believe a careful reading of Hackney (1969) and of Reed (1972) leads to the opposite conclusion.

² This issue, which is not discussed by Dixon and Lizotte, is recognized by Ball-Rokeach (1973, p. 736, n. 1), who writes, "Although Wolfgang and Ferracuti focus their attention on subcultural determinants of homicide, there is no compelling theoretical reason why their thesis, if valid, should not also apply to less extreme forms of violence." Her position may be reasonable for evaluating Wolfgang and Ferracuti's thesis, but it is less justified when applied to Hackney, Gastil, and Reed.

³ Dixon and Lizotte interpret question 5 to measure support for violence in defense of property, but respondents may easily understand it to include defense of family members present in the home.

American Journal of Sociology

Conclusion

Although our remarks have emphasized points of disagreement with Dixon and Lizotte, their call for researchers to include measurements of southern culture that are independent of region of birth or socialization in their models should be heeded. How should researchers proceed? At this time, we can do no better than repeat Hackney's proposal for "a survey of attitudes toward violence, perceptions of the environment, feelings of personal efficacy, and other measures of alienation" (p. 924).

JAY CORZINE

University of Nebraska—Lincoln

LIN HUFF-CORZINE

Kansas State University

REFERENCES

Ball-Rokeach, Sandra J. 1973. "Values and Violence: A Test of the Subculture of Violence Thesis." *American Sociological Review* 38:736-49.

Erlanger, Howard S. 1976. "Is There a 'Subculture of Violence' in the South?" *Journal of Criminal Law and Criminology* 66:483-90.

Gastil, Raymond D. 1971. "Homicide and a Regional Culture of Violence." *American Sociological Review* 36:412-27.

Hackney, Sheldon. 1969. "Southern Violence." *American Historical Review* 74:906-25.

Huff-Corzine, Lin, Jay Corzine, and David C. Moore. 1986. "Southern Exposure: Deciphering the South's Influence on Homicide Rates." *Social Forces* 64:906-24.

Reed, John Shelton. 1972. *The Enduring South: Subcultural Persistence in Mass Society*. Lexington, Mass.: Heath.

———. 1982. *One South: An Ethnic Approach to Regional Culture*. Baton Rouge: Louisiana State University Press.

Wolfgang, Marvin E., and Franco Ferracuti. 1967. *The Subculture of Violence*. London: Tavistock.

THE BURDEN OF PROOF: SOUTHERN SUBCULTURE-OF-VIOLENCE EXPLANATIONS OF GUN OWNERSHIP AND HOMICIDE¹

We are delighted to respond to the issues in the comments of Corzine and Huff-Corzine and Ellison and McCall. The issues they raise are important. However, we are not persuaded by their criticisms of our article.

Hoping to rescue southern subculture-of-violence explanations of homicide, Corzine and Huff-Corzine ignore our findings and rely instead

¹ The authors thank David F. Greenberg, Robert Max Jackson, and David McDowell for helpful suggestions and comments.

Commentary and Debate

on research using designs incapable of answering the questions at issue. On the other hand, Ellison and McCall choose to reinterpret our findings. Corzine and Huff-Corzine's first argument maintains that our findings have no implications for southern subculture-of-violence explanations of homicide because we do not analyze lethal behavior. We never claim to analyze lethal behavior. Our research focuses on three questions: (1) Is there empirical evidence of a southern subculture of violence characterized by social values supportive of violence? (2) If there is a southern subculture of violence, is gun ownership a central feature of that subculture? (3) If there is no southern subculture of violence, is gun ownership a central feature of nonregional subcultures of violence? In our article, we demonstrate how the research design we use in addressing these questions and the answers we derive from our findings affect interpretations of previous gun-ownership findings. Although we do not test a homicide model, we discuss the implications that our research design and findings have for interpretations of previous findings about regional differences in homicide. We will briefly reiterate these implications.

First, previous research assessing the role of subcultures of violence and structural factors in explaining both gun ownership and homicide uncritically accepts imprecision in both theory and research designs. Because recent formulations of the original subculture-of-violence thesis equate region with subcultures of violence, they fail to distinguish between structure and subculture and gloss over the distinctions among violent subcultures and such other types of subcultures as defensive subcultures. Hence the validity of the interpretations of regional effects is threatened by unmeasured structural variables related to region, as well as by unmeasured subcultural variables associated with region but unrelated to subcultures of violence. Since region is used as a measure of subcultures of violence in both gun ownership and homicide designs, it poses a validity problem in both models. Unlike previous research designs of gun ownership, our design introduces a direct measure of the subculture of violence that is independent of region. Thus, we can separate the effects that region, several structural variables, and one aspect of subcultures of violence, that is, violent attitudes, have on firearms ownership (p. 401). Because the research designs predicting homicide have not introduced a measure of subcultures of violence independent of region, it is not clear whether reported effects of region are because of unmeasured structural variables, subcultures of violence, or other aspects of southern culture. As we state in our article, the homicide models need further specification before regional effects can be interpreted as evidence of southern subculture-of-violence explanations of lethal behavior.

Second, we find no significant relation between region and the violent attitudes indicative of subculture-of-violence membership. This casts

American Journal of Sociology

doubt on the validity of the southern subculture-of-violence thesis. This negative finding suggests that there is no special southern subculture of violence based on violent values. Consequently, it increases the need for those using southern subculture-of-violence explanations of any phenomena, including homicide, to support their theses with solid evidence. As we see it, southern subculture-of-violence explanations of gun ownership and homicide assume that region is significantly correlated with subculture-of-violence indicators. Since our data show no significant correlation between region and violent attitudes, we find no evidence for a regional subculture of violence based on violent attitudes. The degree to which other aspects of a southern subculture of violence are correlated with region is unknown, since previous designs fail to include indicators other than region. We do not conclude, as the commenters would have their readers believe, that there are no other aspects of southern culture or subcultures of violence that affect gun ownership or homicide (p. 401). Rather, we remain agnostic until these other aspects are specified, measured, and entered into models predicting gun ownership and homicide. It is not enough to interpret *a posteriori* any significant relation between region and values as evidence of a southern subculture of violence.

In a second argument, Corzine and Huff-Corzine and Ellison and McCall misconstrue our criticism of the current literature by implying that we criticize only the research that supports the subculture-of-violence perspective, for example, Reed's (1972, 1982) research. They are partially correct. We do criticize the interpretations of previous research supporting subculture-of-violence explanations. However, we also criticize the interpretations of previous research refuting southern subculture-of-violence explanations in favor of structural ones. The weak designs of the previous research provide no valid evidence for either perspective (p. 388). In our design, the objective measure of subcultures of violence independent of region enables us to explore in greater detail the relation between subculture-of-violence membership and region.

In addition to misconstruing our literature review, the commenters misrepresent our interpretation of the finding that region is related to defensive attitudes but unrelated to violent attitudes. Although we find that some aspects of culture, that is, defensive attitudes, are related to region and gun ownership, we also find that some other aspects of culture, that is, violent attitudes, are neither southern nor related to firearms ownership. We also find evidence that some, but not all, structural variables affect gun ownership. Corzine and Huff-Corzine and Ellison and McCall suggest that the significant relation between defensive attitudes and region supports Reed's proposition that regional differences in homicide are rooted in an exaggerated defensiveness among white southerners and give evidence of a southern subculture of violence. But this

Commentary and Debate

blurs the distinction between defense and violence. Our findings are consistent with Reed's supposition but inconsistent with the southern subculture-of-violence thesis. Our model, with its independent measures of defensive attitudes and region, offers a better test of Reed's supposition than his own. Our work refines rather than refutes cultural explanations of gun ownership. Concomitantly, we do not suggest that this implies that structural explanations are unimportant.

Our research strategy differs from the approach of Corzine and Huff-Corzine and Ellison and McCall in two fundamental ways. First, we do not infer that lack of evidence for one cultural explanation discredits all other cultural explanations. We do not "unwittingly" find evidence for a southern subculture of violence, as Ellison and McCall state. Rather, Ellison and McCall "unwittingly" misinterpret our findings. Since the defensive attitudes are not even slightly correlated with the violent attitudes indicative of subcultures of violence, we must conclude that they are different aspects of culture with different relations to region and gun ownership. Second, we consider structural and subcultural explanations as complementary rather than competing explanations. Thus, it is not incongruous that some structural and some subcultural factors are both important.

In a third argument, Corzine and Huff-Corzine chastise us for using Wolfgang and Ferracuti's indicators of a subculture of violence rather than those of Hackney, Gastil, and Reed. They argue that our measure of subcultures of violence is too narrow. We find the conceptualizations and measures of Hackney, Gastil, and Reed so ambiguous and imprecise that they severely hamper measurement and interpretation. Since Corzine and Huff-Corzine imply that Hackney, Gastil, and Reed offer evidence for the southern subculture-of-violence thesis, let us explain why our findings are more robust and persuasive.

While Hackney (1969) argues that a southern worldview defines the physical and social environment as threatening, he indicates no empirical referents of this worldview except the high homicide and low suicide rates in the South. If homicide and suicide rates are used to define the southern subculture of violence, then it is a tautology to explain regional differences in homicide with the southern subculture-of-violence thesis.

Gastil (1976) argues that a violent tradition in a wide range of situations condones lethal violence or indirectly raises the murder rate by the value it places on the ready availability of firearms. Gastil does not, however, specify the conditions under which southerners condone violence and others do not. Moreover, while Gastil distances his regional concept of the subculture of violence from Wolfgang and Ferracuti's emphasis on norms and values, he then stresses the "value" southerners place on the ready availability of guns. Are these values unconscious? If so, how do we

American Journal of Sociology

measure them? He measures the southern subculture of violence by an index of historical migration patterns of white southerners. This is essentially a measure of region similar to Hackney's. These two measures have a correlation of .68 (Loftin and Hill 1974). Replications of this index are impossible since no formulas or elaborations of the procedures used to assign scores to states are given.

Finally, Reed submits that southerners will be more violent in those circumstances in which the culture permits or demands a violent response. Similarly to Hackney and Gastil, he does not specify the circumstances that permit or demand a violent response. Reed's anecdotal accounts suggest instances where southerners may act violently, but he gives no evidence that nonsoutherners would act differently. While interesting, these anecdotes are not systematic enough to permit development of a subculture-of-violence scale.

The final and weakest argument of the commenters focuses on our attitude scales. Corzine and Huff-Corzine are "perplexed" by our assertion that the violent-attitude and defensive-attitude scales measure fundamentally different constructs. Their "careful reading of the questions" in the scales suggests to them that both scales measure merely "low-level violence." Our quantitative analysis of the data and its interpretation in terms of our original discussion suggest otherwise. Affirmative answers to questions in the defensive-attitude scale imply that the respondent is willing to defend women, children, and home from unwarranted aggression. This defense of women, children, and home from unwarranted aggression is a mainstream American cultural attitude that most respondents, both southern and nonsouthern, approve. These are mainstream values that the vast majority of people support. It is true that southerners are more likely to support the defensive attitudes than nonsoutherners. This may be indicative of a defensive southern subculture centered around protection of women, children, and home. The violent attitude items do not enjoy such widespread support. Most people reject the use of violence in these situations. So, there is widespread support for defending women, children, and home, with southerners more supportive than others. But there is not widespread support for violent attitudes, and southerners are no more likely than others to exhibit these violent attitudes.

Statistical analyses of the items corroborate this interpretation. If, as Corzine and Huff-Corzine and Ellison and McCall assert, the defensive- and violent-attitude scales both measure "low-level violence" alone, then the correlation between the scales should be high, and items in one scale should correlate highly with items in the other scale. Instead, both a factor analysis and a measure of discriminant validity for the scales show that they are unrelated. Furthermore, the items in each scale correlate

Commentary and Debate

highly with other items in that scale and do not correlate highly with items in the other scale. This suggests that they measure fundamentally different constructs, as we assert in our article. The distinction is important. Without distinctions between violent subcultures and other subcultures, it is possible to interpret the relation between region and any attitude as evidence for a southern subculture of violence.

Because research in this area has focused on testing the merits of structural versus subculture-of-violence explanations of gun ownership and homicide, debates have often revolved around the interpretation of findings as supportive of one or the other of these competing explanations. Both our original article and the comments are limited because of an overconcern with this debate. The finding in our article that structural variables are related to subcultures of violence leads us to believe that the focus of research in this area needs to change. Rather than focusing on either structural or subcultural explanations, future theory and empirical research should explore the interplay of structural and subcultural forces as they converge to affect gun ownership and homicide.

We encourage supporters of the regional conceptualization to specify the situations in which southerners uniquely engage in violent responses, develop a systematic scale measuring these aspects of a southern subculture of violence, and enter them in the model developed in our article. Until a measure of aspects other than Wolfgang and Ferracuti's (1967) are entered into a model with region and structural variables, we will not be persuaded by their criticisms. The burden of proof is with those advocating alternative conceptualizations and measures.

JO DIXON

New York University

ALAN J. LIZOTTE

SUNY—Albany

REFERENCES

Gastil, Raymond. 1976. "Is There a Subculture of Violence in the South?" *American Sociological Review* 36:412-27.

Hackney, Sheldon. 1969. "Southern Violence." *American Historical Review* 74:906-25.

Loftin, Colin, and Robert H. Hill. 1974. "Regional Subculture and Homicide." *American Sociological Review* 39:714-24.

Reed, John Shelton. 1972. *The Enduring South: Subcultural Persistence in Mass Society*. Lexington, Mass.: Heath.

_____. 1982. *One South: An Ethnic Approach to Regional Culture*. Baton Rouge: Louisiana State University Press.

Wolfgang, Marvin, and Franco Ferracuti. 1967. *The Subculture of Violence*. London: Tavistock.

COMMENT ON VARENNE'S REVIEW OF *MIDDLE AMERICAN INDIVIDUALISM: THE FUTURE OF LIBERAL DEMOCRACY*

Hervé Varenne's thoughtful review of my book (*AJS* 94 [May 1989]: 1517-18) emphasized its discussion of the individualism-community debate, but because my reference to that debate was just one of the themes of my book, I would like to comment on the review by telling *AJS* readers some of what else is in the volume. I see my book as developing an argument, although many sociological generalizations and observations are part of that argument. The work is not meant to be a scientific study, but as I pointed out in my presidential address to the American Sociological Association, I believe strongly that sociologists have the right and obligation to discuss public issues, should use sociological data that can contribute to the public understanding of these issues—and have the right to be social critics.

My book's principal subject is a largely structural analysis of the distinctive individualism of working- and lower-middle-class Americans. That individualism consists to some extent of typical values of self-development and personal freedom on the part of a population not so long ago released from conformity to parental and community demands. Middle American or popular individualism is distinctive, however, because at its center is a defensive form of self-reliance, usually for the good of the family, by people who have little effective economic or political power in the country or over the government agencies, workplaces, and other organizations that play a major role in their lives.

As a result, middle Americans feel they must rely on their own efforts. Accordingly, they fashion a life spent with parts of the family and a variety of chosen informal groups, but they treat formal organizations with what I call organizational avoidance. They stay away from such organizations altogether when they can, and when they cannot, as in the workplace, they shield themselves as much as possible by setting up informal groups of colleagues, in this case to control the work and to gripe collectively about superiors.

Organizational avoidance expresses itself in many other ways. All other things being equal, middle Americans prefer goods over services and, among the latter, commercial to professional and governmental ones. As long as they can afford them, goods, like most commercial services, give them more freedom of choice. Organizational avoidance even contributes to a trend toward social decentralization, illustrated by people's moving to nonurban and nonsuburban parts of the country, where, among other things, formal organizations are weakest. This trend is developing concurrently with and perhaps also because of increasing organizational centralization, as corporations, government agencies, and

Commentary and Debate

even nonprofits grow larger, with ever more centralized decision-making structures.

I am particularly interested in *political avoidance*, and the book describes the various ways in which middle Americans keep their distance from government, and especially politics, which they distrust intensely even at the smallest local level. In addition, people are able to insulate themselves from political information they do not need or want—something professionals in education and the mass media still do not understand properly. The avoidance of political organizations manifests itself even in voting, which appears increasingly to be becoming an “upscale” activity.

Intellectuals have frequently been critics of popular, and other, forms of individualism, and my discussion of their critiques involves me in the individualism-community debate Varenne refers to in his review. As part of my “defense” of popular individualism, I note that for middle Americans, appeals for community may suggest a return to the involuntary conformity they have just left behind or demands for joining in the squabbling of local politics or appeals for collective action led by elites that they have not chosen. Actually, the longest part of this analysis is a critique of the concept “society,” not so much as used by sociologists but by interest groups, experts, or others who claim to speak for society.

In the concluding chapter of the book, the emphasis turns from individualism to democracy. I argue that if we want to achieve more representative democracy, as I do, we cannot wait for increased voting or other forms of participation. Instead, I suggest that if people do not come to participate in their political institutions, these institutions must come to them, at times literally. I propose a number of possible complements to existing kinds of representation, including the wider reliance on lobbies, but also new ones that would represent ordinary people in some of their various roles and would provide some balance to the corporate and professional lobbies that now dominate Washington.

Another complement is the expansion of polling, on the principle that with proper random sampling, polls can reach everyone and are thus more democratic—and representative—than political participation. The polls I have in mind must be less commercial and elite centered than at present; they need to ask people detailed questions about what they want for themselves and the country, for example, and not just how they react to what formal organizations do for and to them. These and other complements to participation can add to, broaden, and enrich the feedback and pressure elected politicians now get from their constituents. My proposals do not even rule out schemes to increase conventional citizen participation, although these schemes normally increase the participation of the already well represented.

American Journal of Sociology

Finally, the book turns to the topic of the subtitle, liberal democracy. Despite their antipathy toward government in general, middle Americans support welfare-state policies if these supply the welfare *they* want. The polls and other evidence suggest that they also support antipoverty programs, provided these do not threaten their own interests, including class and racial ones. I discuss the outlines of such programs, such as forms of income redistribution middle Americans might accept once people know it benefits them rather than serves as a pretext for subjecting them to new taxes, which is what they now suspect.

Still, a more liberal democracy also requires middle America to resist business attempts to convert it to entrepreneurial business individualism, as it already resists appeals for collective solutions from liberals. Indeed, since liberal appeals often reflect the upper-middle-class values of liberal professionals, I argue that if liberals want a more liberal democracy, they have to eliminate those biases from their political and cultural activities.

An appendix, "Methods and Other Matters," discusses the shortcomings of polls to which Varenne refers, but also ways of choosing and reading polls so that they, or at least carefully selected questions in them, can be useful for the analysis of popular attitudes and values.

HERBERT J. GANS

Columbia University

REPLY TO GANS

Professor Varenne has stated that he does not wish to respond.

Book Reviews

American Families and Households. By James A. Sweet and Larry L. Bumpass. New York: Russell Sage Foundation, 1987. Pp. 488. \$39.95.

Arland Thornton
University of Michigan

American Families and Households was commissioned by the National Committee for Research on the 1980 Census as part of the Census Monograph Series, *The Population of the United States in the 1980s*. This monograph series represents the continuation of a long and respected tradition of census monographs beginning in the 1930s. The goal of the series is the production of a comprehensive description of the U.S. population that is informed by a historical perspective.

Jim Sweet and Larry Bumpass provide detailed descriptions of three components of the households and families of Americans: family transitions, including leaving home, marriage, marital dissolution, and child-bearing; the prevalence of different family and household arrangements; and the economic and social circumstances of people living in different types of families and households. As the authors state, the

book takes advantage of the large samples provided by the decennial censuses and Current Population Surveys to document in detail the changing prevalence of different family and household living arrangements. . . . Our intent is explicitly to provide a reference work that will be useful to persons desiring more detailed information than is generally available on some of the most important dimensions of these domains. . . . [The work has] been informed by what we understand to be the major theoretical and substantive issues in family and household research. At the same time, statistical modeling and extensive theoretical discussions are beyond the objectives of this volume—not because we do not care about them but because we judge the present format most likely to be of value to a wide community of scholars over a substantial time period. This is a work in the social demography tradition that regards a careful understanding of what has happened as a necessary condition for explanation. [Pp. 5–6]

American Families and Households achieves its goals well. As a reference work, the volume is a gold mine, with many rich veins of useful information. Just listing the 207 tables and 64 figures requires 17 pages, and many of these tables and figures are complex ones, containing information about numerous subgroups of the population over several differ-

Permission to reprint a book review printed in this section may be obtained only from the author.

American Journal of Sociology

ent years. The authors also provide expert guidance and interpretation through the enormous number of data. Anyone interested in American families and how they have been changing will want to refer to this volume.

Although the general outlines of most of the trends and differentials documented in this volume will be familiar to most family scholars, the large individual level data files from multiple censuses allowed the researchers to provide substantially more information than was previously available. Particularly noteworthy in this regard are the numerous time series of detailed information extending from 1940 through 1980. The authors were also able to provide more detailed information about the families and households of numerous ethnic groups than was available before. In this regard, however, people interested in European immigrant groups will be disappointed to find very little attention paid to them.

People using *American Families and Households* as a reference book about specific topics will want to keep in mind the interconnections among family issues. In the current volume, many themes recur throughout the book. Readers will want to be alert to the possibility of useful material in various portions of the book and be persistent in finding the many nuggets that are located away from the main veins of interest.

Although the book is a gold mine of useful information, it should not be considered a complete description of the American family. Two limitations in scope are caused by the volume's primary reliance on information from the Census Bureau. First, the book is almost exclusively about family relationships within households. The many dimensions of family relationships that occur between kinfolk living in different households are given little attention. Second, the volume is almost entirely about family and household arrangements and behavior. Data limitations preclude the study of interpersonal relationships in families, and there is virtually no information about family attitudes, values, and norms.

Although the book is primarily descriptive, it ends with a provocative and useful chapter discussing the historical roots of current family change and a speculation about the future. The thesis is that many of the sweeping changes that have occurred after World War II are extensions of previous trends and certainly must be understood in the context of other historical, social, and economic transformations. The speculation is that these social and economic transformations have not run their course, and it is unlikely that current family structures and behavior have reached equilibrium. I find myself in general agreement with both points but am painfully aware of how little we are able to link precisely changes in the family with changes in other societal institutions. One hopes that *American Families and Households* will provide motivation for renewed efforts to explain the trends carefully documented in this volume.

Book Reviews

Society and Family Strategy: Erie County, New York 1850-1920. By Mark J. Stern. Albany: State University of New York Press, 1987. Pp. 160. \$44.50 (cloth); \$14.95 (paper).

Kathryn M. Neckerman
University of Chicago

Mark Stern's *Society and Family Strategy* joins other recent attempts to relate American fertility trends to changes in the family economy. Inspired by demographic theory, most notably the work of John C. Caldwell, these studies are based on a simple premise: when children began to work less and go to school more, they became more expensive and parents had fewer of them. But Stern's research departs from other such work in its focus on class. He links the fertility and schooling patterns of different occupational groups to changing strategies of class reproduction, which are in turn driven by structural economic change.

To examine these issues, Stern turns to Buffalo and the surrounding rural areas of Erie County, New York. During the latter half of the 19th century, he writes, Buffalo's economic development led to a bifurcation of both business class and working class. Entry into the new business class was attained through education rather than through inheritance or apprenticeship. The split in the working class corresponded to the primary and secondary labor markets, denoted here by skilled and unskilled labor and unionization of craft and factory workers.

One by one, Stern proposes, these classes adopted a strategy of lower fertility and more investment in education. The new business and professional class used education to ensure that their children would inherit their own status; having smaller families allowed members of this class to pay for schooling as well as for the consumer goods that became increasingly important signs of class membership. More slowly, the old business class adopted this strategy as well, as industrialization and concentration of ownership limited the opportunities for sons to inherit the family business. Rising wages allowed many families in the skilled working class to spare the labor of their children and to send them to school to prepare for jobs in the growing white-collar sector. Only the unskilled working class, composed mostly of immigrants, had little incentive to limit their fertility and send their children to school. These low-wage workers still needed child labor to supplement the family income. In addition, because many immigrant unskilled workers did not plan to stay, they preferred short-term financial gains over investment in their children's future in the American labor market.

Stern's empirical analysis, based on samples from the Erie County manuscript censuses of 1855, 1900, and 1915, largely supports his argument. Fertility declined first among families in the new business class and last among unskilled workers, with the other classes falling in between as expected. However, the effect of class on fertility is reduced when ethnic-

American Journal of Sociology

ity is controlled, and within ethnic groups, fertility levels of different classes do not always rank order as Stern predicted.

Education is an integral part of Stern's story. He examines several key questions in the literature on education and the family economy. He argues, for instance, that the simultaneity of the decline in fertility and the rise in school attendance implies that education influenced fertility through its effect on the cost of children, not (as some argue) on the values of adults. Stern also notes ethnic differences in patterns of school attendance, but he concludes that cultural influences on educational choices did not long resist structural economic change. The traditional values of immigrant parents yielded rather quickly to the incentives of the labor market.

But Stern exceeds his evidence in his assertions about culture and class consciousness. For instance, he writes that, although incomes rose and white-collar employment expanded gradually throughout the 19th century, "the working class 'saw' the transformation of society around it and moved suddenly to take advantage of it" (p. 114). Stern's quantitative data are too crude to permit such a nuanced interpretation; further, his assertions about working-class consciousness are made without empirical support and without mention of what might have stimulated such a sea change in attitude. Here and elsewhere, he calls on culture mainly to explain anomalies in his data.

Stern raises far more questions than he can answer. In so short a book, his presentation of theoretical perspectives sometimes approaches caricature, and he leaves the reader in some doubt about what his own argument really is. His quantitative analysis has a parallel fault: he does not exploit his data set to its full potential, presenting few multivariate results and relying instead on a baffling array of one- and two-way tables.

Stern's contribution lies in his lively (if contentious) theorizing and his relentless sifting and testing of diverse perspectives. His work draws from, and thus will inform, disparate literatures on class formation, the fertility transition, and the family economy. And his blunt, clear prose deserves emulation. Although his empirical analysis is far from definitive, the connections he traces between industrial development and the family strategies of different classes delineate important directions for research in this field.

Women Adrift: Independent Wage Earners in Chicago, 1880–1930. By Joanne J. Meyerowitz. Chicago: University of Chicago Press, 1988. Pp. xxiii + 224. \$29.95.

Lisa M. Fine
Michigan State University

Recently, during a discussion with a colleague of mine who happens to be a women's historian as well as a historical demographer, we both decided

Book Reviews

that those groups of women who do not fit the standard demographic characteristics of their time and place are often the most interesting groups to study precisely because of their deviance from the norm. Women who blazed their own paths, or were forced to deviate from the majority, not only left a more traceable paper record of their existence but also highlight the boundaries of acceptable behavior and the limits and power of cultural prescriptions. Joanne J. Meyerowitz's *Women Adrift: Independent Wage Earners in Chicago, 1880–1930* proves these hypotheses extremely well.

Meyerowitz's study centers on women who lived apart from family in the city of Chicago between 1880 and 1930. Most women during this period lived in some kind of household with kin; therefore, focusing on women who lived apart from kin reveals an exceptional and often short-lived experience of independence. Nevertheless, Meyerowitz believes that "women adrift" were the vanguard for many changes in action and attitude of 20th-century women. The variety of their experiences highlights the possibilities for the exploitation of women as well as the potential for women's community and agency.

After a thorough description and discussion of the characteristics of this group of women, Meyerowitz examines the prevailing cultural images of women adrift and the efforts of private and public organizations to protect and meet the needs of these women. Ever mindful of differences in race, age, marital status, and occupation, Meyerowitz tells the story of how the image of the woman alone in the city, in need of protection (of herself and her sexual purity), evolved by the 1920s into a symbol conveying "urban energy and female sexuality" (p. 132).

One of the great strengths of the book is Meyerowitz's examination of the various subcultures of women adrift. For some, the experience of being on their own was overwhelmingly frightening and dangerous. Naïve, vulnerable, and usually poorly paid, many women had little or no material or emotional reserves and sometimes resorted to prostitution or other forms of "sexual service" to make ends meet. For others, independence meant liberation from abusive family situations, restrictive Victorian values, or simply boredom. Some women joined together in apartments or houses, as many college students do today, for mutual financial and emotional support. Some of these women sought jobs in the sexual service sector as taxi dancers or cocktail waitresses because of the freedom and relatively good pay. Some sought the excitement of the growing rooming-house districts. Lesbian and radical women, as well as other men and women who desired urban anonymity, found refuge and companionship in these quarters. The description and discussion of these subcultures not only provide an important corrective to the often-assumed overwhelming hegemony of Victorian morality but also contribute to the revision of scholarship on the revolution in manners and morals that was supposedly accomplished by middle-class youth in the 1920s.

Meyerowitz's study also fits nicely into another recent body of scholar-

American Journal of Sociology

ship in women's history. Attempting to navigate a middle course between women's history scholarship that depicts all women as victims of overwhelming patriarchy and scholarship that paints women in the past as nascent feminists, recent historians such as Susan Porter Benson, Barbara Melosh, Alice Kessler-Harris, Christine Stansell, Kathy Peiss, Jacqueline Jones, and Linda Gordon have described how women exerted power over the course and direction of their lives. As Linda Gordon states, "We need concepts of male supremacy that can explain the power that women have managed to exert, the more impressive because emanating from such disadvantaged positions . . . and the extremely complex struggles, negotiations, and cooperation with which the sexes have faced each other and the social/cultural institutions that define gender relations" (*Heroes of Their Own Lives: The Politics and History of Family Violence* [New York: Viking, 1988], p. vi). This study of women apart from family contributes to this worthy mission.

Unfortunately, Meyerowitz is not self-conscious enough about her place in this scholarship. In her introduction, she places her study in the debate "concerning the meaning of wage work for women" and does cite the most recent contributions to this scholarship (pp. xxi–xxiii), but her three-page conclusion is unsatisfying after such rich material. Her comparison of the subjects of her study with present-day Third World women, who are leaving their families and homes in increasing numbers for work in multinational corporations, is even more provocative and begs the larger question: What does the story of women adrift tell us about the continuing relationship between capitalism and patriarchy? Women adrift may have been more than "a vanguard in the decline of Victorian culture"; they may have heralded a new variation of world capitalism (p. xxiii).

But Meyerowitz's analytical oversight only underscores for us the richness and possibilities of her material. *Women Adrift* is a carefully documented, well-written, and attractively produced addition to the ongoing scholarship on women's past experience in America.

Putting on Appearances: Gender and Advertising. By Diane Barthel. Philadelphia: Temple University Press, 1988. Pp. ix + 219. \$19.95.

Ella Taylor
University of Washington

The adman responsible for marketing Betsy Wetsy (a plastic doll that wets its pants) takes credit not only for bringing "inordinate joy to a little girl for a few dripping moments" (p. 24) but also for creating employment for everyone involved in making and selling the doll. This is not all. Betsy Wetsy, in his view, falls into the same aesthetic category as the Sistine Chapel ceiling ("not essential, but they do improve the quality of life" [p.

Book Reviews

25]). Diane Barthel knows how to make bald quotations provide their own social critique; we are simultaneously entertained and appalled by what she calls the “cultural reductionism” of commercial language. Barthel uses a ready wit and a sharp eye for the absurd to great sociological advantage in this lively, articulate study of advertising and gender, *Putting on Appearances*.

For Barthel, as for others, advertising is rooted in the consumerism on which capitalism relies, but it is not simply a capitalist plot designed to deceive innocent publics. Advertisements offer narratives that, like other aesthetic forms, draw on the values of Western culture but co-opt and transform them in order to make profit. Using the work of Roland Barthes, Jean Baudrillard, and Georges Bataille on the signifying power of symbols as well as feminist theorists and anthropologists Victor Turner and Mary Douglas on symbolic exchange, Barthel looks at advertising as a relationship between subjects and objects that serves to shape male and female modes of “gendered” relationships.

Echoing John Berger’s analysis of advertising and painting in *Ways of Seeing*, Barthel believes that advertising is about appearances, the harnessing of goods and services to social identity and status. But it is also about gender, for the sound business reason that women still command 80% of purchasing power even in these days of marketing demographics. Her interpretation of advertisements in a sample of leading women’s magazines from 1979–81 goes beyond conventional content analysis to an examination of ads as thematic narratives that build on the fantasies and desires of women and men and convert them into wealth for those who make and promote products. The bulk of her book is divided into short but impressively documented chapters, each of which excavates different aspects of the gendered messages sent to women readers by magazine ads. “The Accursed Portion,” for example, dissects the rhetoric of “bad body parts” and offers a stinging indictment of the ways in which weight-loss ads prey on women’s anxieties and force them to turn their critical energies inward rather than out into public activity. “A Gentleman and a Consumer” examines a smaller sample of ads in 1987 men’s magazines and shows that, while male and female modes are becoming a little more arbitrary in American culture, they remain sharply differentiated in the *identity of consumption*, with the male mode connoting power and freedom, and the female a passive complacency.

From the standpoint of gender studies, Barthel’s most suggestive arguments concern the use advertising makes of social change. In “Women in a Man’s World,” she extends historian Carroll Smith-Rosenberg’s discussion of the changing idea of the “New Woman” to the language of advertising in the 1970s. Contemporary advertisements conflate earlier images of the New Woman as moral crusader and sexual confidante into one unproblematic role: “Armed with such new slogans as ‘assertive, not aggressive,’ and new beauty charms (the magic briefcase), women can make their mark on the world without leaving a mark on their colleagues

American Journal of Sociology

and lovers" (p. 124). Similar versions of this superwoman abound in movies and television, I have noticed, and, as Barthel points out, the contradictions of assuming multiple roles in a man's world are glossed over or eliminated.

Returning in her final chapter to the broad sociological concerns raised in her introduction, Barthel argues that advertising is not the source of all capitalism's ills but its messenger. The problem is not that it exists but that, increasingly, advertising becomes the only ideological show in town, a privileged discourse that, in the absence of cultural alternatives, jumps into the gap created by the decline of community and substitutes for collective involvement in family and public life.

Every textual analysis calls for an audience study to bridge the gap between the interpretations of "expert" and "naïve" readers. An ethnography of how women—and men—talk about advertising and consumption would complement Barthel's rich, complex mining of her material. We live in knowing, hip times, when every consumer, let alone social scientist, understands that she is being maneuvered by the language of advertising to see herself as a particular kind of woman (indeed some of the more urbane ads openly acknowledge consumer skepticism) *and may buy the products anyway*. The question is why, but that is not Barthel's brief. *Putting on Appearances* is such a good read that it makes an excellent text for undergraduate or graduate courses; it also offers a spirited contribution to the growing literature on the social significance of advertising as well as to the study of the social construction of gender.

Women of Academe: Outsiders in the Sacred Grove. By Nadya Aisenberg and Mona Harrington. Amherst: University of Massachusetts Press, 1988. Pp. xii + 207. \$30.00 (cloth); \$10.95 (paper).

Mary Jo Deegan
University of Nebraska—Lincoln

Women of Academe contains many powerful and moving accounts of 62 women who work in or on the margins of the male academy. Although the women earned doctorates and received the highest training in their disciplines, they are outsiders to the patriarchal rules of the game. They neither speak nor think with male authority. The painful reality of being excellent in one's field yet always "not good enough" lies at the heart of this book.

The topic of this book is central to our profession, and, for many of us, including myself, it addresses our work, ideas, and everyday lives. Academic women have reached a ceiling to achievement and acceptance, reflected in the fact that only 6% of the full professors at universities in 1983 were women (p. 5). Women, moreover, were 36% of assistant professors and 52% of instructors in that year, while by 1986 there was more than a 5% differential in the salaries of male and female professors (p. 6).

Book Reviews

Nadya Aisenberg and Mona Harrington elicited sensitive and complex information from their sisters and organized this information into a series of major topics. First, they find women caught between two scenarios: "a marriage plot," enacted through domestic life, and "a quest plot," enacted through a search for knowledge. Socialization into the intellectual journey involves a "transformation" of the female self in graduate school, but the "rules of the game" in academic life are not taught to women then or later as faculty. The very self-presentation of women militates against "the voice of authority," and this problem is aggravated by "women's work" in low-rank positions, often in low-prestige institutions with low salaries and heavy teaching loads. Women's everyday lives and encounters with domestic burdens conflict with the "life of the mind." "Countervalues and change" must be internalized and institutionalized in order to remove the entrenched barriers against women in the academy.

The authors clearly understand the personal problems facing academic women, but they do not understand structural solutions or barriers. They have repeated the process of doing "women's work" in this book. I quote their analysis of their respondents' work to criticize this book: "This is, therefore, a classic story of 'women's work' in the academy—choosing humane subjects, placing subject matter in a cultural context, transcending disciplinary boundaries, seeking social change through transformed consciousness, and paying the price" (p. 158).

Again, their subjects' very difficulties are those of the authors: academic women "have strong misgivings about using institutional authority to which they have gained access" (p. 59). The authors are in a position of authority and power, yet they pull back from analysis, criticism, or controversy. Similarly, they rightly point out that critiquing women colleagues is very difficult for women academics because it is "traitorous" to our joint marginality—my very situation as a book reviewer.

Self-reflection, like this book and my review, can reveal only part of the problems confronting women in the academy. Thus, the authors could have explicitly stated their idealist theory; articulated the anger their respondents must have felt; deeply questioned why tenured women, untenured women, and unemployed women perceived similar realities despite their dramatically different locations in the academy; and wondered why women of privilege felt powerless.

The women interviewed make up a highly biased sample. They were all white women, none identified herself as lesbian, one-half were graduates of Ivy League institutions, and only 11 of the 62 respondents were trained outside the east coast. Although half of academic women do not have children, two-thirds of this sample did. Many respondents referred to professional husbands, and no respondent talked about hunger, homelessness, or falling back on parental help. Similarly, none of the respondents appeared to be Marxists or strong critics of the political and economic structure of this country.

Clearly, this book addresses some of the most important problems facing this society and our profession: equality in everyday life, full em-

American Journal of Sociology

ployment, and respect for a variety of ideas. Academic women need more than mentors, plans, publications, and tenured positions—some of the authors' recommendations for change. We need, for example, effective affirmative action, including quotas; access to funded legal resources; specific plans for tenuring part-time positions so that a permanent under-class is not created; powerful analyses of hegemonic knowledge and its linkage to capital, prestige institutions, and "hard" data; and joint demands for equality across race, age, and gender. This type of structural critique is beyond this book. The authors fail to move beyond the limitations on women's work that their subjects depict.

First Births in America: Changes in the Timing of Parenthood. By Ronald R. Rindfuss, S. Philip Morgan, and Gray Swicegood. Berkeley and Los Angeles: University of California Press, 1988. Pp. 291. \$35.00.

Beth Osborne Daponte
University of Chicago

In this compelling book, Ronald Rindfuss, S. Philip Morgan, and Gray Swicegood successfully synthesize the abundance of literature on life-course transitions and the changing American family structure while addressing questions surrounding the transition to parenthood. Particularly, they examine the timing of this important transition using an original conceptual framework and analyze a variety of data sources.

The conceptual framework of *First Births in America* includes period or macro factors; race, ethnicity, or nationality; and microsocial-structural factors. They examine the effect of period changes and their interactions with micro-level factors. The framework does not include marriage as an independent variable. Also, they examine the transition to parenthood within a life-course framework, emphasizing role succession and complementarity.

The authors regard the transition to parenthood (defined as the first live birth) as important for two primary reasons. First, they use the term "normative parental imperative," meaning that the normative pressure to become a parent is strong and pervasive. The normative parental imperative comes from society's and the family's concern with reproduction and is manifested in regarding the transition to parenthood as the transition to adulthood. Second, the transition to parenthood involves permanent and continual obligations.

I found their argument about the importance of examining in detail "on-time" parenthood persuasive. They cite that "on-time" parenthood is most sensitive to changes in social and economic conditions. In addition, they describe two scenarios of "on-time" parenthood—in the first, the

Book Reviews

mother and father are 19 and 21 years old, respectively, and in the second, they are 24 and 26. The writers note that, although the two couples differ by only five years, the experience (knowledge, maturity, and education), resources, and skills differ considerably. This dissimilarity could affect the subsequent life courses of the couples and their children.

To make comparisons of the timing and propensity of the transition between birth cohorts, races, and two countries, they use a variety of data sources. In chapter 4, they draw on these sources to examine overall trends in the transition to parenthood in the United States. Although most of their analyses on the transition to parenthood include only white women, Rindfuss et al. include two chapters that use comparative analyses. They devote chapter 6 to racial (black-white) similarities and differences in the United States. To see how the American pattern of parenthood compares with that of non-Western societies, the authors include a chapter contrasting the timing of first births in Japan and the United States.

I find the conceptual framework innovative. The authors see no necessary causal relationship between marriage and the birth of a child. They justify this framework by citing the propensity of premarital conceptions and out-of-wedlock births and by stressing that the decision to marry and the decision to bear children may be joint. They reject the "classic" situation—after marriage, a couple decides to have a child. In summary, on this topic they write:

Thus, what is the role of marriage in the transition to parenthood? Our argument is that it is variable. There is no necessary relationship between marriage and becoming a parent. Note that we are not arguing that marriage is either irrelevant or unimportant for childbearing; rather its importance varies across socially defined groups, as does the causal direction of the relationship. Given this situation, most of our analysis focuses on the timing of the first birth without reference to marriage. We do so because conceptually the first birth is a far more precisely defined and measured variable, and because the transition to parenthood (particularly motherhood) is a far more permanent transition. Further, just looking at those who have a first conception after marriage means that a substantial number of people are being excluded. [P. 37]

First Births in America: Changes in the Timing of Parenthood should be read by anybody interested in changing patterns of fertility and family formation. The authors provide an excellent summary of previous work, an innovative framework to examine the transition to parenthood, empirical analyses of the transition, and, finally, a reevaluation of their conceptual framework and development of a comprehensive model of the transition.

American Journal of Sociology

The American Census: A Social History. By Margo J. Anderson. New Haven, Conn.: Yale University Press, 1988. Pp. xiv + 257. \$30.00.

William Petersen
Carmel, California

Very few of the social scientists, historians, and journalists who depend on the census as an empirical foundation for their arguments have any idea of how the data were collected and compiled or, thus, of how much confidence the figures deserve. Nor, on the other hand, have special interest critics of the counts typically made an effort to acquaint themselves with how the Census Bureau works or what political, financial, and bureaucratic constraints limit its operations. There is a vast body of writings on such questions, but virtually all are too technical for the general reader, too partisan to be recommended, or narrowly concerned with one topic or one period. *The American Census* was written to fill a conspicuous gap, and on the whole it has accomplished this very well.

As is well known, the Constitution itself prescribed a regular count so that the number of congressmen from each state could be adjusted periodically to the growth and movement of the population. Apportionment, however, depends not only on the number of people but also on how one chooses the ratio between that population and the number of its representatives, and Margo Anderson's book opens with a very interesting account of the recurrent debate on that issue. Subsequent chapters follow in chronological order with analyses of how the census was affected by the crisis over slavery and its aftermath, the mass immigration from the 1880s to 1914 and its effect on population composition, the Great Depression of the 1930s and the growth of a welfare state, and the development of new tools for counting, classifying, and interpreting. The book ends with a few tables of pertinent figures, a short bibliographical essay, and an index.

The best chapters are those on the 19th century, when opposed views were sharply defined and the technical questions were rather simple. In contrast, some of the confusions permeating disputes about the 1980 enumeration are reflected in this account, and such new statistical techniques as "editing" and "imputation"—the weakest link in the Census Bureau's defense of its current operations—are barely mentioned. The chapter on the most recent enumeration ends with a flabby refusal to evaluate that count, leaving it "open" whether it was "the beginning of a truly new concept of statistical service" or "an expensive white elephant" (p. 235). As a historian, Anderson is understandably least comfortable in trying to assess the most recent period.

Every one of the censuses was embedded in disputes that, to one degree or another, affected the results. Anderson is eminently fair in presenting both sides of these debates; her bias is rather that of an analyst who wants data to work on. She never faces up to the question underlying all the

Book Reviews

others: how much effort, money, and invasion of privacy are warranted to produce an ever-larger quantity of figures. Each cut in the agency's budget is represented as unfortunate, if not reprehensibly partisan. Social research, however, is likely to come closest to the truth when it is conducted in a bias-free environment, and social data, comparatively, are least reliable when they pertain to hotly contested questions, most accurate when no one cares about the answers. When the slaves were emancipated in 1862, Anderson notes, northern policymakers acted in "abysmal ignorance and misinformation with respect to black Americans" (p. 57). Remarkably, she cites the works of Roger Fogel and Stanley Engerman together with those of their often vitriolic critics to show that even today, a century later, one can come up with figures to compare agricultural productivity based on slave versus nonslave labor (p. 56). The point is rather that neither then nor now has it been possible to agree on an adequate empirical base for rational policy on this or various other broad questions. In her account, similarly, the 1850 census—a monstrous experiment in collecting farfetched data hand compiled years after they might have been of some use—"proved successful" (p. 53). And Francis Walker, who in 1870 supervised the worst count ever taken in this country and later used his scholarly reputation to lead a racist attack against immigrants, is summed up as one who "became a nationally respected expert on statistics, economics, and public policy" (p. 78).

Apart from such criticisms, this is a worthwhile book, an excellent summary in language that the general reader can understand of how the country's statistical record was shaped by American history. If those who use census figures take the time to read it, the overall level of their discourse will rise appreciably.

Migration and Politics. By Thad A. Brown. Chapel Hill: University of North Carolina Press, 1988. Pp. xxi + 198. \$29.95.

Rodolfo O. de la Garza
University of Texas at Austin

Americans are an extremely mobile people. Annually, 20% of the nation change residences, and 75% do so at least once during their lives. Previous research has concluded that this movement has had little effect on individual political attitudes, voting behavior, and the body politic. Following a careful and well-crafted analysis of 1952–80 National Election Study surveys, with particular attention to the 1970 and 1980 surveys, and the Youth-Parent Panel Study of 1965–73, *Migration and Politics* challenges those findings and concludes that "internal migration has pronounced effects on citizens' political actions and loyalties and beliefs" (p. 4).

Overall, this is a clearly written study, organized into three sections.

Thad Brown begins with a review of political migration theory and two descriptive chapters on migrants and migrant streams. Part 2 focuses on the socializing effects of political environments and how migration from one environment to another may expose individuals to new types of socialization that could effect partisanship and electoral behavior. Although political environments are described as "the total of all politically relevant events, information and social interactions that surround an individual" (p. 72), for well-justified reasons the study operationalizes them as congressional districts. Congressional districts are then identified in terms of partisan characteristics, and migration is measured by movement from one partisan type of political environment to another. This approach makes it possible to determine the effects of migration on "those migrants whose political environment remained unchanged and those for whom it did change" (p. 71). This approach is an important advance over previous research that did not control for such changes. Part 3 includes an analysis of the effects that mobility has on attitudes toward policy domains and partisanship. The study concludes with a succinct recapitulation of the major findings and an all-too-brief statement on how migration is "unraveling the party system."

Among the study's salient findings are that migration affects political attitudes and behavior gradually (the effects are seen after 11 years) and that it affects migrants between 18 and 35 more than older migrants. It also found that migrants who experience incongruent migration (i.e., move from one partisan environment to a different one) are most likely to consider self-interest rather than partisan cues when voting.

The overall significance of this research is diminished because of a lack of theoretical and conceptual clarity. Brown dismisses the significance of variables such as motivation, ambition, and risk-taking proclivities, which international migration research shows are dimensions along which migrants and nonmigrants differ, with the assertion that the presence of such attributes "would dictate a response (change) immediately after a migration" (p. 16). At minimum, this is an overstatement.

Equally troubling is some confusion about the study's focus. Overall, the study correctly defines its subject as the unintended political consequences of migration. On page 29, we are told that "Internal migration is not usually a political act," and throughout it is clear that Brown does not consider the migrants analyzed to be political migrants. Nonetheless, part 1 is titled "Political Migrants," and chapter 1 is titled "A Theory of Political Migration." Since neither in these sections nor elsewhere does the study seriously address political migration, these titles are misleading and inappropriate (as is much of the argumentation on pp. 7–10).

From another perspective, however, there may be more political dimensions to this migration than the study acknowledges. For example, migrants may explain their movements as an attempt to change the social, racial, or economic character of their environments. To the extent that those environments have been shaped by governmental policies,

Book Reviews

such movements could be considered political migration. Here, these are defined as economic or social migrations. The study also takes no account of intraparty variation in measuring migration from one type of political environment to another. That is, all that is measured is changes in partisanship between environments; no attention is given to changes in the meaning of partisanship across locations. Yet many southern Democrats could have moved North or West, changed parties, and never changed their beliefs or attitudes. Although the data do not permit analyses of these types of questions, these issues should have received more attention in the conceptual sections of the volume.

Overall, then, *Migration and Politics* is a useful study whose value would have been enhanced had more attention been given to fundamental theoretical and conceptual issues. Its primary value is that it offers useful insights into the unintended political consequences of migration and thus also makes a potentially important contribution to the literature on adult socialization.

Children of Circumstances: Israeli Emigrants in New York. By Moshe Shokeid. Ithaca, N.Y.: Cornell University Press, 1988. Pp. 226. \$28.50 (cloth); \$9.95 (paper).

William B. Helmreich

City College of New York and CUNY Graduate Center

Israeli immigrants to the United States, commonly referred to as *yordim* ("those who go down"), have been coming to the United States since 1948, shortly after the State of Israel was founded. Today the group numbers an estimated 300,000 persons, residing, for the most part, in New York City and Los Angeles. Despite their numbers, they have not been the subject of much research by social scientists.

In this penetrating and insightful book, Moshe Shokeid, an anthropologist by training, uses the tools of his field and of sociology to provide a portrait of this community. Shokeid spent two years in Queens, New York, where there is a large concentration of Israelis. The core of his involvement was his participant observation at a social club frequented by *yordim* and his interaction with Israelis in other settings.

What emerges in *Children of Circumstances* is a complex picture of a group ambivalent about its role in the larger American Jewish community and its place in American society. Shokeid discusses these feelings at length and the patterns of social interaction among the *yordim* themselves. Along the way, we learn how they try to maintain their identity and how their experiences in the United States have affected their perceptions of themselves as Jews.

One unique aspect of this community is its failure to develop any ethnic organizations or associations, unlike many other immigrant groups.

American Journal of Sociology

Shokeid attributes this to a desire to deny the permanence of their stay in America, while noting that the lack of such organizations weakens the community's ability to help its members advance in this country.

Most of the emigrants were not pushed out of Israel because of financial problems. Rather, they came here because they wished to improve their situations. In addition, many had no intention of staying when they first arrived. Changing circumstances (usually, developing opportunities) caused them to change their minds.

One source of resentment toward *yordim* by American Jews is the view that the emigrants are traitors who have forsaken family and nation in search of the "almighty dollar." Condescension by American Jews is only part of the problem facing *yordim* in their struggle to adapt. More fundamental, perhaps, are the differences between American and Israeli cultures, and it is here that Shokeid's book is most valuable.

Most of the complaints voiced by emigrants revolve around the impersonal nature of American society, the difficulty of developing friendships, and the failure of many people they meet to invite them into their homes. By contrast, Israeli society is seen as a place where people are warm and giving, and informal. Whether deriving from real cultural differences or simply homesickness, such perceptions are functional, and Shokeid explains why: "Their sense of real or assumed rejection by American Jews supports the self-image of *yordim* as closer to the Israeli axis of identity in spite of their present status as Diaspora Jews" (p. 50). In this way, Israelis are able to maintain a sense of close identification with the homeland they feel so guilty about leaving. They are still true Israelis.

On the other hand, *yordim* reveal a certain degree of self-hatred common among many minority-group members. Stung by the dominant culture's criticisms, they respond at times by rejecting their own and stigmatizing them. Shokeid's respondents are often highly critical of other Israelis, accusing them of nosiness, greed, and unreliability. Because Israelis tend, as Shokeid points out, to view their own society in Israel as altruistic, it seems appropriate to ask why this shift takes place in the United States.

Among the Sephardic Jews there is considerable involvement with the Lubavitcher Hasidim, and Shokeid presents a fascinating explanatory discussion linking key elements of these two cultures in ways that are both novel and illuminating of sociological principles and theory. His discussion of politics and why *yordim* prefer the Likud to the Labor party is similarly enlightening.

Shokeid also demonstrates why substantial numbers of Israelis' leaving the "Promised Land" is functional for American Jews who feel guilty about not having fulfilled the Zionist dream of settling in Israel. As Shokeid puts it, "If the Israelis themselves prefer to live in América (argues the American Jew), why on earth should American Jews move to Israel?" (p. 212). One might note that those American Jews who wish to remain loyal, if only in theory, to the Zionist dream, may view the matter

Book Reviews

somewhat differently. Some might see the Israeli immigrant as a person who has turned his back not only on his country but also on his family and life-long friends.

Precisely because Israeli immigration to the United States in significant numbers is a recent phenomenon, the adaptation of this group will have to be measured over time with respect to such matters as the identity of the *yordim's* children, their organizational affiliations, their interactions with American Jewry, and so on. Because Shokeid immersed himself so ably and deeply in the life and culture of these people, future researchers will have an excellent foundation on which to build further studies and investigations of this unique and important group. Moreover, the work is firmly anchored in the theoretical writings of Eisenstadt, Simmel, Glazer, and others, thus making it an ideal text for minority-group and social stratification courses.

Familiar Strangers: Gypsy Life in America. By Marlene Sway. Urbana: University of Illinois Press, 1988. Pp. xi + 155. \$19.95.

Beverly Y. Nagel
Carleton College

Gypsies are among the least known ethnic groups in our society. The most common response when the subject of Gypsies comes up in conversation is, "You mean they still exist?" Indeed, they do. In *Familiar Strangers*, Marlene Sway examines the mechanisms that have worked to prevent the assimilation of Gypsies into mainstream society (especially in the United States) and ensured their survival as a distinct ethnic group throughout history and in a variety of social and economic systems. Through this case study, she aims also to "review and refine" middleman minority theory, which she has adopted as an analytical approach to understanding Gypsy persistence. Underlying these analytical objectives is the desire to present a sympathetic description of the cultural traditions and daily life of this much-romanticized and much-stigmatized group.

Sway would appear to be an ideal person to undertake this task. *Familiar Strangers* begins with what anthropologists call the "authenticating anecdote," in which Sway recounts how she came to know the Gypsies as a child and how her contacts with these old friends were maintained over the years. These personal ties provided a strong foundation of mutual trust and empathy that is essential in any fieldwork, but especially with a group whose relationships with outsiders have so often been dominated by fear, hostility, and persecution. In addition, Sway draws on material gathered during briefer visits to Gypsy communities in Virginia, as well as archival and comparative material.

This introduction whets the reader's appetite. One anticipates a richly detailed study that can illuminate our understanding of Gypsy life by

American Journal of Sociology

setting new ethnographic data within a comparative and theoretical context. Although the book does address each of its major goals, it does not fulfill this promise.

Most surprising is the relative absence of the author's field observations in the text. The book is organized in a conventional ethnographic format, with chapters on Gypsy origins and history, religion, family, legal system, and economics. But the material presented in these chapters is drawn largely from secondary sources, with only occasional snippets from the author's own fieldwork. The most systematic use of the author's primary observations are to be found in the sections dealing with courtship and with the *kris* (Gypsy legal system).

While the relative absence of the author's own field observations is merely puzzling, other omissions weaken this book's contribution to the literature on Gypsies. Despite her drawing heavily on secondary sources, Sway ignores major parts of the small, but growing, literature on Gypsies. No serious attempt is made to summarize key analytical and methodological issues or identify what the key differences among Gypsy groups are. Instead, the review tends to oversimplify or overstate sometimes tangential arguments from a few studies. Equally troubling are the sources that are missing from the descriptive chapters as well as from the review. Only one source on Gypsies published after 1980 is cited. Sway overlooks Matt and Shelia Salo's work on Gypsy occupational strategies and concepts of ethnicity and completely ignores Carol Silverman's insightful work on impression management and ethnic boundary maintenance in the practice of fortune-telling.

Sway's main analytical innovation is the application of middleman minority theory to explain Gypsy persistence. The book demonstrates convincingly that the experience of the Gypsies fits this model. In her concluding chapter, Sway seeks to "review and refine" the model, showing how Gypsies survive as a middleman minority "beyond the status gap" across a variety of social and economic systems. A secondary point is that a sojourner ideology is not an essential facet of middleman minority persistence. Neither point is likely to be contested by middleman minority theorists, who have noted similar patterns among groups ranging from Chinese to Armenians.

In the end, Sway identifies the Gypsies' nomadism, economic adaptability, and strong (often ethnocentric) cultural tradition as key to Gypsy persistence as a "perpetual minority." This is surely a valid conclusion and clearly fits the well-established tenets of middleman minority theory. But however valid this conclusion may be, it will not come as a surprise to Gyniologists, who have identified similar processes using other analytical frameworks.

Although Sway has demonstrated the usefulness of middleman minority theory as a means of organizing data about Gypsies, there is little else that is new here. For readers unfamiliar with the literature on Gypsies, this book provides much useful information, presented fairly and sympathetic.

Book Reviews

thetically. It is here that Sway is at her best. But for these readers, some parts of the discussion may be too cursory, and they will miss the ethnographic coherence and richness of other works. In sum, though *Familiar Strangers* brings together much useful information, it is in the end disappointing.

Hewers of Wood and Drawers of Water: Noncitizen Arabs in the Israeli Labor Market. By M. Semyonov and Noah Lewin-Epstein. Ithaca, N.Y.: Cornell University Press, 1987. Pp. 168. \$25.00 (cloth); \$10.95 (paper).

Saskia Sassen
Columbia University

At the time of Israel's occupation of the West Bank and Gaza Strip in 1967, there were only a few thousand noncitizen Arabs employed in Israel. By 1982, there were 75,000 registered noncitizen Arab workers from the Gaza Strip and the West Bank. As of 1969, registration was a requirement for all these workers, but it is estimated that by 1982 there were an additional 35,000 who were unregistered and hence illegally employed in Israel. Israel's census and employment service data show that registered Palestinians from the occupied territories are almost exclusively concentrated in low-paying and un- or semiskilled jobs. These are workers that are not allowed to reside in Israel and must return to the West Bank and the Gaza Strip every day. And they are registered to work for a specific employer.

Beginning with a detailed presentation of these facts, M. Semyonov and Noah Lewin-Epstein apply a wide range of models and measures, largely from the U.S. stratification literature of the 1960s and 1970s. The purpose is to understand characteristics and determinants of the labor-market status of Palestinians from Gaza and the West Bank and to understand the incorporation of subordinate ethnic groups in a labor market. A brief description of one of the core chapters of *Hewers of Wood and Drawers of Water* provides a good idea of the types of analyses the authors have chosen. Chapter 2 is on the ethnic structure of the Israeli labor market, the occupational segregation of noncitizen Arabs, and changes in this occupational segregation over time. Explanations about the emergence of ethnically segmented labor markets provide the theoretical framework. There is a discussion of the ethnic succession model, the queuing model, the more social psychological explanation advanced by Williams in the 1940s on the relationship between minority population size and discrimination, and other classic models of this literature. The empirical units of analysis are occupational categories defined at the two-digit level.

The authors apply two distributional measures to the data, the index of dissimilarity and the index of occupational concentration, to evaluate the

American Journal of Sociology

ability of noncitizen Arabs to penetrate the occupational structure in Israel. Next is the Lieberson index of net differences to establish whether differences in occupational distribution between noncitizen Arabs and other groups in Israel are systematically related to status ranks. Then the authors examine the determinants of differential participation using cross-product odds ratios. The results of all these analyses show a uniformly low and worsening position for noncitizen Arabs. At this point, the authors find it essential to estimate the net effect of each of four variables on the rate of participation of noncitizen Arabs in the labor market: ethnic composition, unemployment rate, percentage of salaried workers, and average age of workers in an occupation. The central politicomilitary fact that shapes the condition of Palestinians from the occupied territories in Israel is simply not considered. The chapter continues to subject the available evidence to more and increasingly elaborate statistical procedures. And this is only one chapter. The other three chapters that form the core of the book follow a similar logic of explanation.

The book provides us with a first-rate illustration of various sociological models and statistical procedures that can be used to understand the incorporation of ethnic minorities in a labor market and their chances for occupational mobility. Some of these models are indeed powerful tools that would allow one to disentangle differences between such groups as Italian and Polish workers in Chicago or Colombian and Mexican immigrants in the United States. But the choice of population limits the explanatory power of these methods and models. It becomes a way of trivializing both the condition of the workers and such sociological models. The methods and procedures used basically pivot on issues of socio-economic status and rank and are intended to capture detailed variation. But there is not much variation in the status and rank of noncitizen Palestinians employed in Israel: they are segregated and concentrated in low-income occupations, and their absence from high-status occupations is absolute. All the results confirm what the authors had presented in the first 15 pages of the book with evidence from Israeli government statistics.

Work and Industry: Structures, Markets, and Processes. By Arne L. Kalleberg and Ivar Berg. New York: Plenum Press, 1987. Pp. 244. \$24.95.

Randy Hodson
Indiana University

Work and Industry seeks to integrate recent research in the United States on the sociology of work. The goal is to provide both a scholarly integration and a teaching tool for graduate courses on the sociology of work.

The authors' primary conceptual contribution is the development of a matrix of six work "structures" cross-classified by six types of markets. The six work structures, or factors that influence the nature of work, are the state, classes, occupations, industries, business organizations, and unions. The six types of markets are product markets, capital markets, resource markets, demand for labor, labor supply, and political markets. The literatures that Arne Kalleberg and Ivar Berg argue can be organized within this matrix include organizational and labor-market analysis, stratification research, industrial sociology, and occupational sociology, not to mention economics, history, political science, industrial relations, and social psychology.

Kalleberg and Berg argue that previous studies of work are flawed if they do not include a consideration of all of these factors. Thus, they criticize studies that consider only one factor and commend those that consider several factors. Any given study of work would not have to include all these factors, but it should at least be organized so that comparisons across the various dimensions are possible.

The purpose of the matrix is to guide researchers in situating their studies and encourage them to consider as many factors as possible in their research designs. This is to be done either by including measures of the factors or by selecting cases that allow for comparisons across dimensions that do not vary within a single study. Such integration is an admirable goal advanced by the book.

Another stated purpose of the book is to encourage greater attention to the connections among different structures and the examination of how these structures influence one another. This is most convincingly argued for the effects of the state on various aspects of work such as benefits, safety and health, and corporate regulation; less so for other connections. However, the invitation to give greater attention to these interconnections is well taken.

The most fundamental limitation of the book is that a 36-cell matrix is no substitute for an integrative theory of how these structures are interrelated. The authors make no attempt to outline such a theory, and without a theory of how these structures are related, the matrix provides a way to label and categorize research articles but little more. I am not sure what such a theory would look like, but one might try to develop a theory of work based on *power*, in contrast to Oliver Williamson's market efficiency model of organizations. The work involved in developing such a theory would be immense. Providing a matrix of factors can perhaps be seen as a preliminary step in developing such a theory. But without some beginning statement of that theory, how are we to know whether these are the correct factors to consider?

The matrix can also be criticized on more specific grounds. Kalleberg and Berg's model of work structures and markets is ahistorical. Rapid changes are occurring in the nature of work that, at a minimum, involve technological advances based on the widespread application of micropro-

American Journal of Sociology

cessors, shifts in the world economy, and changes in the nature of women's involvement in paid labor. The matrix of work structures and markets described here is limited in its ability to help us in our efforts to come to theoretical terms with how these important changes are shaping the nature of work.

In addition, each "structure" is itself made up of a complex set of dimensions that are not independent of one another. Organizations and unions, for example, cannot be "added" to class or even "interacted with" class. The identification of these other structures as significant factors involves theories of social organization that take many of the same factors used by class theorists and array them in different ways with different claims about causal priority. Similarly, other aspects of work are not well conceptualized as "correlates" of these six work structures. The components of any one theoretically identified structure are to a significant extent shared by other theories; they are just integrated differently. This complexity is not well captured by the language and conceptualization of a "multivariate" regression model that assumes independence among causes.

In spite of these criticisms, the book might be used in a graduate course on the sociology of work. The coverage is extremely wide, and this might serve as a useful organizing framework for students. The book was written with teaching uses in mind and contains a number of pedagogical devices, such as lead questions, that begin each section.

The endeavor of classifying the growing body of research on work is an important one, and Kalleberg and Berg have made a worthy effort in this regard. They apologize in their preface and at several places in the book for not having a theory to integrate their conceptual model. I wish they had at least begun the work of developing such a theory. Their efforts may, however, encourage greater attention to the connections among the many influences on the nature of work. Their efforts may also encourage the theoretical work needed to conceptualize work adequately and to understand contemporary changes in the nature of work.

The Mobility of Capital and Labor: A Study in International Investment and Labor Flow. By Saskia Sassen. Cambridge: Cambridge University Press, 1988. Pp. xi + 224. \$34.50.

John Walton
University of California, Davis

Any author aspiring to say something new on the subject of migration faces great competition. Classic studies by Max Weber and W. I. Thomas have plowed this furrow. Social scientists from around the world have had a go at it. In the past decade or so, new theories have come in rapid succession, supplanting rude notions of push and pull with heady anal-

Book Reviews

yses of migratory labor systems and ethnic enclave economies. Yet, in this short and densely written volume, Saskia Sassen succeeds at the all-important challenge: she develops a new idea.

The core question in *The Mobility of Capital and Labor* is of how to explain a new and, by conventional standards, contradictory pattern of international migration. Why do emigrants since the 1970s come mainly from the rapidly industrializing countries of Asia, Latin America, and the Caribbean, which have received substantial foreign investment and employment stimulation during the period, rather than from the poorest countries? Why have immigrants to the United States since the 1970s concentrated in a few large cities troubled with growing unemployment, inflation, and, often, deindustrialization? What do these facts imply for theories of migration? Sassen argues forcefully that one tight proposition explains the seemingly discrepant trends: "The same set of basic processes [capital flows and their employment consequences] that has promoted emigration from several rapidly industrializing countries has promoted immigration into several booming global cities" (p. 22). The explanatory power of these processes requires, in turn, a reconceptualization of migration theory in which Third World populations, underdevelopment, and immigration policy are mere intervening factors in the more broadly determinant "articulation" of labor migration and global economic change—notably the internationalization of production and market relations.

The study provides a wealth of descriptive detail, which can be only sampled here. Advanced industrial countries are increasing their use of low-wage immigrant labor, and the United States, in particular, is approaching the record high numbers of the turn of the century. Another major flow is directed to the Arab oil-exporting countries. The main sending countries include Mexico, the Philippines, South Korea, both Chinas, India, and the Caribbean states. Women make up the larger fraction of these emigrants. In the United States, today's immigrants are absorbed mainly in urban services. Asians, not Hispanics, are the fastest-growing of the new immigrants. The importance of these trends, however, lies in the basic processes that link major sending and receiving sites.

The core questions divide into two areas. The first explores conditions for the formation of a migrant labor supply in the sending countries—the curious situation of emigration from countries with high industrial growth rates and foreign investment. In brief, four circumstances explain this fact. Industrial growth in the Third World has occurred because of foreign investment in firms devoted to export. Ironically, however, rather than reducing unemployment in the underdeveloped sending countries, the prototypical export-processing zones have mobilized new segments of the population into wage labor, notably young women. This new form of internationalized export manufacture has disrupted traditional work structures in the sending countries. Not only are rural households divided

American Journal of Sociology

and young men left without employment and women, but the careers of women in export zones are short-lived because firms prefer docile and agile youth. In combination, these circumstances create a growing pool of potential migrants who, Sassen speculates, favor a move to the advanced countries because of ideological and cultural attractions over returning to traditional communities of origin.

Second, and key to the argument, the same processes of internationalization explain the demand for immigrant labor in the global cities of advanced countries. Sassen focuses on the apparent Sunbelt–frost belt polarity of Los Angeles and New York, arguing that in fact both cities are the leading beneficiaries of the new immigration because, despite New York's decline in some economic sectors, both cities are fast becoming centers of global management and service; polarizing into high-wage jobs in finance, corporate management, advertising, and global services, on one hand, and low-wage jobs in garment manufacture, restaurants, production for specialty shops, building attendance, dog walking, cleaning, and the whole informal economy, on the other hand. The older middle sectors are disappearing in global cities. Perhaps the most compelling feature of Sassen's book is the resonant analytic foundation it provides readers familiar with the daily commerce of cities like New York and Los Angeles.

The book has its flaws, too. Most important is the diffuse and uncharacteristically confusing chapter on historical forms of migrant labor. Sassen emphasizes correctly that migratory labor systems are varied and changing historical categories, that today's phase of internationalized production evolved from earlier ones, yet the exact "stages" or "phases" are not sharply characterized or placed in a sequence governed by some fundamental principle. As a result, the historical chapter yields a disappointingly flat and unconvincing contrast between all "earlier phases of capitalist penetration and incorporation" (p. 52) and the author's richly nuanced treatment of our current phase. Similarly, the author promises more than once to give us "a partial reconceptualization of what has been called the informal sector" (pp. 24, 129), but I did not find this made explicit anywhere. Oddly, the volume closes with the suggestion that class struggle or labor militance is a key determinant of the capital mobility that has orchestrated the whole analysis. Up to the last page, collective action by labor receives only passing and frankly speculative attention.

Having accomplished one kind of explanatory analysis rather brilliantly, Sassen might have ended it there, or sold us on her next book with the lure that a very different analysis would flow from standing capital on its head and collective action on its feet. In any case, this is a fine study, a model for further historical and political treatments of a subject that Sassen has done much to refresh.

Book Reviews

Falling from Grace: The Experience of Downward Mobility in the American Middle Class. By Katherine S. Newman. New York: Free Press, 1988. Pp. xiv + 320. \$22.95.

Stephen J. McNamee

University of North Carolina at Wilmington

Falling from Grace draws attention to a fact of social life that has been repressed in the American culture: downward mobility. As Katherine Newman indicates, however, downward mobility is pandemic, affecting as much as 30% of the American population. This book differs in many ways from most mobility monographs. It is written from the perspective of an anthropologist rather than a sociologist. It focuses on downward rather than upward mobility. Mobility is examined intragenerationally rather than intergenerationally. It employs qualitative rather than quantitative methods. And it focuses on the subjective meaning of the experience itself rather than its objective incidence.

Newman examines four downwardly mobile groups—skidding white-collar business executives, air traffic controllers fired by Ronald Reagan as a result of the Professional Air Traffic Controllers Organization (PATCO) strike, blue-collar workers laid off as the result of the closing of the Singer Sewing Machine plant in Elizabeth, New Jersey, and divorced women scrambling to recover economically in the wake of failed marriages.

To study the experience of the skidding white-collar business executives, Newman immersed herself in the world of members of the Forty Plus Club of New York City. Even in the face of unemployment, members of this self-help support group attempted to retain some of their exclusivity by restricting membership to former highly paid executives and professionals. Newman vividly describes the psychological struggles of the members to maintain self-respect in a culture that has defined them as failures. Self-blame was rampant, even in situations in which the members were victims of larger external forces clearly beyond their control.

Unlike the business executives who had been let go one at a time, the PATCO strikers were fired en masse by Reagan. The PATCO workers, most of whom had working-class backgrounds, fell faster and farther than the former business executives. Yet this group did not engage in the self-blame of the business executives. Instead, they saw themselves as having taken a principled stand. In addition to complaints about being underpaid, the PATCO strikers' grievances extended to public safety issues. They objected to the use of faulty equipment and oppressive schedules, which they saw as undermining public safety. In justifying their civil disobedience, they adopted the righteous moral rhetoric of the civil rights movement. Throughout their ordeal, the PATCO strikers maintained a high degree of social solidarity. They had trained together, worked together, and gone on strike together. This strong degree of group

solidarity prevented the slide into self-blame more characteristic of members of the Forty Plus Club.

The circumstances of "falling from grace" were also different for the blue-collar factory workers in the Singer plant. They had been laid off in a progressive "slow bleed" until the plant was shut down altogether. But it was not just their jobs that were at stake but a whole way of life that had developed in this New Jersey factory town over the course of the 100-year history of the Singer Company. In this case, workers attributed the blame to misguided management, a decline in emphasis on craftsmanship, and the erosion of a traditional system of ethnic nepotism. Workers avoided self-blame but at the same time could not admit the primary reason for their plight—the decline in demand for the product they produced.

For divorced women, the loss of economic status was compounded by the emotional disappointment of failed marriages. Here, Newman noted a distinctly different set of coping strategies for divorcees of the Great Depression generation than for their younger counterparts. The Depression generation women, who had previously experienced economic hardship, had better survival skills. The younger women floundered more economically, although they were more likely to remarry eventually. Both groups sought economic support from ex-husbands, but they interpreted this support differently. The younger women who had come of age during the period of women's liberation more often resented being economically dependent on their ex-husbands, whereas the older women were more likely to see this support as just compensation for earlier service as homemakers.

Although each of these different types of downward mobility had different causes and was interpreted differently, there were some common elements. Those who fell from grace felt anger, dismay, and a strong sense of betrayal. Their lives were disrupted and their self-worth called into question. The meritocratic culture offers few psychological supports for the downwardly mobile to draw upon to make sense of their predicament.

Overall, this book is a superb treatment of a neglected, yet important, aspect of mobility. Those who are looking for a definitive treatise on downward mobility will be disappointed in this book, but Newman nevertheless succeeds in shedding new light on a neglected area in the otherwise voluminous mobility literature.

Book Reviews

City, State, and Market: The Political Economy of Urban Society. By Michael Peter Smith. New York: Basil Blackwell, 1988. Pp. x+242. \$34.95.

John R. Logan
State University of New York at Albany

Urban scholars in the new "critical" tradition have become wary not only of neoclassical economism but also of the Marxist position that has informed much of their thinking in the past decade. There is concern that Marxism is too strongly grounded in its own economic theory, that it has not adequately taken account of increasing state intervention in the economy, and that it imprudently discounts the importance of the experiences, impressions, and struggles of little people. There is no consensus about the details of this critique, however, and it is not yet clear what kind of theory will emerge from the current ferment.

City, State, and Market, by political scientist Michael Peter Smith, clearly aims to push forward this debate. Smith argues that, while economics is important, urban theory must address more directly the "historically specific social and political processes through which economic forces must work" (p. 6). The capitalist economy, he points out, works differently in the United States and Western Europe, leading to quite different patterns of urbanization and different allocations of costs and benefits. Politics and culture are the filters through which economic forces produce the city. And, in the United States, these include such diverse phenomena as exclusionary zoning, the legitimization of homelessness, the geography of military spending, and national political party strategies. The problem, then, is to find a theoretical model within which market, politics, and culture can be systematically treated.

Smith describes the politicization of capitalist economies as "welfare state capitalism." The traditional conception of the welfare state involves government assistance to the poor. Currently, Smith believes, the welfare state's benefit programs have little positive effect on social inequality. Further, traditional welfare expenditures are far exceeded by "tax expenditures" in the form of subsidies provided by the American tax code. These subsidies, in turn, generate new forms of inequality (between people, between regions) and new sources of social conflict.

Even more broadly, he argues that welfare state capitalism takes large sectors of the economy out of the "supply and demand discipline" of the market, including certain promoters of urban growth and the high-technology military bloc. At this point, the argument resembles that of free-market advocates, but Smith is headed in a different direction. Because the state's role is indirect and relatively "invisible," it is safe from challenge despite being socially regressive. Smith's point is that the urban and fiscal policies of the state profoundly politicize the economy, but in a manner that eludes public scrutiny.

American Journal of Sociology

Another major section of the book addresses the ideological bases and policy choices of what Smith calls "market capitalism," including a lengthy and interesting analysis of Milton Friedman's work. He attacks Friedman for ignoring the inherent political bias in any pattern of state action, including inaction, and shows how "free-market" ideology has been used to legitimate welfare retrenchment during the Reagan years.

A concluding section reviews tendencies toward globalization of the economy, and its consequences for the growth, decline, or restructuring of American cities. It is here that Smith's emphasis on urban policy gives him at least modest grounds for optimism. In the face of a destabilization of daily life for the newly unemployed and their neighbors, disproportionately minority-group members, he anticipates a new political mobilization. Mirroring Castells's theory of urban social movements, he suggests that "the residential community has become the shopfloor of workers in the informal sector," creating "considerable potential for mass mobilization and political instability" (p. 220).

This book has unusually broad theoretical scope and addresses a wide range of real-world phenomena. It demonstrates clearly that "urban" research is relevant to major topics traditionally categorized as stratification, ethnicity, political sociology, social movements, and so on, and that interdisciplinary thinking is working in this area. The critique of structural Marxism and ideas about directions in which it needs to be extended are provocative and worth consideration. The emphasis on the ideological basis of Reagonomics is a welcome addition to the usual analyses of winners and losers.

I share Smith's premise that the urban future is not determined by economics. But I am more cautious about the prospects for changing the current direction. It is not obvious that policy choices can work against the grain of economics, regardless of who is in power. And altering state policy depends on changing the present balance of political force and belief. In the end, Smith relies on a potential mobilization from below, for which there is precious little evidence.

Socialist Entrepreneurs: Embourgeoisement in Rural Hungary. By Ivan Szelenyi, in collaboration with Robert Manchin, Péter Juhász, Bálint Magyar, and Bill Martin. Madison: University of Wisconsin Press, 1988. Pp. xi + 255. \$35.00 (cloth); \$15.95 (paper).

Steven Brint
Yale University

In an era of startling developments in the socialist world, surely the most surprising has been the spread of capitalist institutions—particularly, the slow expansion of market sectors where entrepreneurship, however limited, can begin officially to flourish. Ivan Szelenyi and his collaborators,

drawing on many years of fieldwork and detailed life-history surveys from the early 1980s, have given us the best study to date of the kinds of people who become agricultural entrepreneurs in the most market-oriented of the Eastern European societies, Hungary. *Socialist Entrepreneurs* is, in many respects, a model work of social science, combining rich ethnographic observation, valuable theoretical contributions to the comparative study of class structure and social mobility, and ingenious empirical work designed to test competing theories about the kinds of people who become entrepreneurs.

The authors concentrate on four positions in the rural social structure: cadres (officials) and proletarians (who occupy positions exclusively in the collectivized system of agriculture), entrepreneurs, and peasant workers (who produce for family consumption and perhaps a little for the market, but primarily work for wages in the collective system).

Against those who have interpreted entrepreneurship in the countryside as a vestige of a traditional way of life or as a province mainly of very small-scale peasant workers, Szelenyi and his collaborators put forward a theory of "interrupted embourgeoisement." They hold that the leading stratum of entrepreneurs are people who came, not from the most depressed or traditional backgrounds, but from among those most committed to entrepreneurial activity in the years before the Soviet occupation. They are people who found "parking orbits" in the social structure, where they could preserve a degree of autonomy and gain skills while resisting full-scale cadreification or proletarianization.

The empirical study, which is based on impressively detailed life-history surveys conducted by the Central Statistical Office in 1982–83, shows support for this thesis. The study also finds support for demographic and more traditional causes of family-based production: commitment to older forms of rural life, associated with older heads of households, and the sheer availability of adult bodies in the household to help with the labor.

Szelenyi's contributions to the comparative study of class structure come in his ideas about the simultaneous operation of "rank-order" and "market-based" class systems. In pre-1944 Hungary, the traditional rank-order system was composed of landlords, the genteel middle classes, the landed peasantry, and manorial laborers. After 1944, this was replaced by a new rank-order system—the "redistributive-bureaucratic order"—with the cadre elite at the top and collectivized workers below. The market sector, with its classes, entrepreneurs, and wage laborers, was subordinate in pre-1944 Hungary, and it almost entirely disappeared in the 1960s before reviving in a very restricted way in the 1970s.

Szelenyi describes the major social mobility trajectories as leading toward gentrification in the old rank-order system or toward cadreification in the new rank-order system, toward proletarianization, or toward embourgeoisement in the subordinate market system. He argues that these major mobility paths (and also more refined paths) have varied in a

American Journal of Sociology

systematic way since 1944 by class of origin and by elite policy at the time of a cohort's arrival at adulthood.

Ethnographic detail from the field studies complements the theoretical and empirical work. The authors show, for example, how the "square house" serves as a symbol of success for the agricultural entrepreneurs. These houses become a tangible sign of the possibilities of the second hierarchy in rural Hungary.

For all of its virtues, the book is not well organized. This is a pity since it should have many readers. The authors would have been well advised to include an introductory chapter for nonspecialists on the structure of rural society in Hungary, both pre- and post-1944. Although the statistical analyses are elegantly designed, the discussion of the analyses sometimes slips into an unappealing "techspeak" and at other times into the eccentricity of a presentation of a sort of horse race among variables. Many of the tables could have been reserved for an appendix.

But it is the excellence of the book that should be emphasized. It is a measure of the book's power that it not only tells us about socialist entrepreneurs but also provides an interesting looking glass for reflections on the origins of agricultural capitalism in the West and even on the current class structures of Western societies.

It is worth mentioning that Szelenyi concludes with a brief, thought-provoking reconsideration of his book with George Konrad on intellectuals in socialist societies. With the rise of the second economy, intellectuals are, in Szelenyi's view, apparently now stalled on their road to class power. Indeed, Szelenyi looks forward not to rule by reforming intellectuals but to contention between the leaders of the command economy and leaders of the emerging market economy, with some increasing freedom and opportunity for those who can play one kind of master off against the other. And so a book that begins with the (qualified) celebration of entrepreneurship ends with the discovery of another venerable Western theme—the virtues of plural centers of power.

Of Rule and Revenue. By Margaret Levi. Berkeley and Los Angeles: University of California Press, 1988. Pp. x + 253. \$35.00.

John Markoff
University of Pittsburgh

Despite the frequency with which social scientists claim to be studying "the state" these days, taxation remains remarkably understudied. The distribution of resources by governments in the form of welfare has been the subject of a great deal of research, the extraction of resources much less so. Yet taxation systems impinge on many of the same issues: the development of governing structures, class relations, conceptions of citizenship, the interaction of governors and governed. Gabriel Ardant's

Book Reviews

kaleidoscopic work has stood for some time in splendid isolation. (Are tax codes drearier to read through than welfare statutes?)

In the past few years, there are some indications of discovery of this vital but unknown territory, and Margaret Levi's *Of Rule and Revenue* is part of this new interest. This study has a central thesis, argued through an examination of case studies as far removed in time as the tax farming of the Roman Republic and the income tax of 20th-century Australia, but it also presents itself more generally as an advertisement for the models of rational choice the author favors. Levi asks us to consider rulers who "are predatory in that they try to extract as much revenue as they can from the population" (p. 3). Her revenue-maximizing rulers must deal with the costs of enforcing and monitoring compliance. The ruler uses agents who may cheat or shirk. Taxpayers may be more willing to pay if they feel they are contributing to a public good but are always leery of being suckers who bear the costs that others evade.

Levi explores taxation as a problem of collective action. Taxation systems appear as continuously renegotiated deals between rulers, agents, and taxpayers. One critical variable is the relative bargaining power of actors: a feudal lord with a private army and an impregnable castle is hard to collect from. A second is the cost of measurement. If the economy does not make much use of money, the central bureaucracy does not reach below county-size units, and the peasants cannot fill out forms (but can hide their pigs), an income tax is not to be expected. A third element is the ruler's time orientation: those with long time horizons favor tax practices that will not kill the goose that lays the golden egg, but those under pressure (usually the military) try to take what they can get today.

Perhaps the most inventive concept Levi deploys is "quasi-voluntary compliance." Some citizens may be willing to pay ("voluntarily") for what they see as a service of the ruler, but only so long as all pay since the spectacle of free riders makes volunteers feel like fools. To obtain the willing assent of some, then, the state coerces others.

The case studies show that it is possible to employ this conceptual apparatus to reveal interesting things about differences among and changes within systems of taxation. By virtue of its conceptual clarity, this book is likely to influence the terms of discourse of future research. I would like, therefore, to comment on some of the limits of this discourse.

Levi sees ideological commitment as a motivation to comply or avoid. Lying "outside the model of rational choice," as she puts it, such a "non-rational, irrational, or imperfectly rational" (p. 50) element is treated as exogenous to her model. Ideational matters, however, seem part and parcel of her own central concepts. Free riders, for example, play a critical role in her analysis. But the identification and even the existence of free riders may be contested terrain. Who is supposed to pay, what is regarded as a benefit, and what is regarded as payment are questions that may be differentially perceived, subject to conflicting judgments, and fought over by propagandists. If nobles are exempt from payment, have

American Journal of Sociology

they filled their public duties by contributing to war? If a church does not pay property taxes, is the state failing in its duties to compel payment, or is it fulfilling its duties to support the religious grounding of social life? Levi's stimulating book leaves such issues for someone else and takes as given such a sentiment as "all should pay": the problem is that "all" and "pay" are subject to social definition and redefinition. Levi shows that the economist's tool kit needs to be modified in the direction of greater realism by introducing political considerations. In this case it strikes me that attention to political culture is an essential direction for still greater realism. I hope that Levi's work triggers discussion of the boundaries of her models rather than merely inspires the probably inevitable applications to other cases.

The Social Origins of Political Regionalism: France, 1849–1981. By William Brustein. Berkeley and Los Angeles: University of California Press, 1988. Pp. xiv + 243. \$28.00.

John Markoff
University of Pittsburgh

William Brustein's work deals with the problem of regional continuities in political allegiance, an important topic for historical sociology as well as a classic theme of French history. Since his argument in *The Social Origins of Political Regionalism* on the roots of the distinctive political colorations of western and Mediterranean France differs from some recent lines of argument, it should excite much interest. One of the most noteworthy interpretations of western conservatism, for example, has it that the conflict of the 1790s was so intense that communities that divided then have remained divided; in short, much of western France was frozen into a tradition of supporting the political Right. As for the Mediterranean south, it is currently argued that political conflicts around the middle of the 19th century locked many localities into a sort of reflexive leftism.

Brustein is skeptical of explanations grounded in regional political cultures. He contends that persistence of voting patterns can be explained by enduring constellations of material interests that support Right or Left allegiances. The varying social structures of the countryside led cultivators in different regions to understand their interests differently. Western France, with its high levels of tenancy, dispersed settlements, isolation from the towns, and politically active class of well-off landholders, was a setting in which a stratum of patrons controlled the resources vital to a dependent rural clientele. This clientele could see that its interests were bound up with the local upper strata and voted for the Right even when the secret ballot freed them from elite coercion. Contrasting models of the structure of peasant interest explain the political choices of other regions.

Book Reviews

Theoretically, Brustein is committed to the exploration of rational choice models and criticizes explanations that speak of norms, traditions, or "irrational" allegiances of various kinds. But he is also committed to subjecting these models to empirical scrutiny. The dependent variable in his multivariate analyses is electoral behavior; the predictors are indicators of socioeconomic structures. The search for indicators led him to an extensive and successful search for archival or other primary data sources. The evidence consistently supports his model of rational interests. In one ingenious analysis, for example, he casts doubts on arguments for the primacy of self-sustaining regional cultures by showing that sub-regions that depart from the typical regional patterns in economic and community structures also depart from the electoral patterns.

Some of his tabulations, however, do suggest that cultural elements are also at play. Is there a more complex rational choice model underpinning the apparent intrusions of a very different order of phenomena? I believe that the most likely issues to be debated about this book will concern the degree to which all findings can be assimilated to the rational choice perspective. But, minimally, it is clear that the structure of peasant interests (in turn, a function of the local organization of production) explains a lot.

The empirical side of this study is rich enough so that it will interest many not attached to the author's theoretical preferences. Consider, for example, Brustein's test of whether it is religiousness or the "mode of production" (his own concern) that provides the more effective explanation of Left voting. He finds that *both* enter the regression model significantly. More dramatic, the effect of religion is stronger in the west than in the south. Students of the French Revolution, who have been debating for some time the precise role of religiously based conflict in the extensive counterrevolutionary actions of western peasants, will be fascinated.

This work should also prove of interest to scholars investigating the nature of peasant political movements in a comparative perspective. Brustein demonstrates that a good part of rural voting in modern France seems an appropriate strategy to further rural interests. For those who think the people of the countryside are incapable of rational appreciations of their situation and require urban intellectuals to tell them what's what, this book will present a challenge. The ways in which peasant assessments of local conditions are linked to national politics is a central theme of the lively debate touched off by the works of J. Scott, S. Popkin, and J. Paige. This new book will only add to the discussion.

American Journal of Sociology

Constitution Making: Conflict and Consensus in the Federal Convention of 1787. By Calvin C. Jillson. New York: Agathon Press, Inc., 1988. Pp. xiv + 242. \$30.00 (cloth); \$15.00 (paper).

Suzette Hemberger

Princeton University and American Bar Foundation

Calvin Jillson's goal in *Constitution Making* is to put to rest the generations-old debate over whether the framers of the U.S. Constitution followed the dictates of principle or of self-interest. The problem with the debate thus far, he contends, is that we do not know what happened at the convention because no one has done an adequate statistical analysis (p. 34). To fill this gap, Jillson does a factor analysis of the roll-call voting record and identifies five different coalition structures, each of which, for a time, dominated decision making at the Philadelphia convention. Invoking the model of "critical elections," he interprets coalition realignments as the result of changes in the nature of the questions considered by the assembly. He distinguishes between two types of questions involved in constitutional choice and argues that each type produced its own pattern of voting-bloc alignment. "Higher"-level questions, those involving relatively abstract issues of institutional structure, led to the development of coalitions based on principle, while "lower"-level questions, decisions about who gets power, fostered coalitions based on self-interest. Thus Jillson's contribution to the debate over the making of the U.S. Constitution is not simply to demonstrate that the framers were guided by *both* principle and self-interest but also to explain when each motivation governed and why.

Stated at this level of abstraction, the argument seems reasonable enough. Yet any reader familiar with the convention debates will be surprised by the extent to which Jillson's methodological commitments obscure the events he is trying to interpret. This problem can be clearly seen both in Jillson's construction of his data set and in his assessment of "who won, what and when" (p. 200) at the convention.

Jillson constructs his data set from roll-call voting records because such information lends itself to factor analysis. Yet, by relying solely on these records and by treating each roll-call vote as equally important, Jillson distorts what happened at the convention. Not all votes were roll call, and the fact that—or the frequency with which—an issue was put to a roll-call vote provides no indication of its significance. Many roll-call votes concerned relatively trivial matters such as whether to adjourn for the day. Conversely, some highly contested issues occasioned days of heated debate but very few votes. Moreover, every vote was always open for reconsideration, and, because each provision was voted on at least three times over the course of the convention, a number of votes taken in the early stages of the proceedings were ultimately reversed.

Because Jillson refuses to assess the relative importance of the various

roll-call votes, the bulk of votes taken during a period of stability establishes the coalition structure. As a result, there is no guarantee that a particular alignment will explain the resolution of the “critical issue” that supposedly generated it. For example, Jillson’s second cleavage, which develops over the issue of representation, shows that the small-state coalition was outnumbered by the large states. Yet the small states *won* equal representation in the Senate. Why? Because “Massachusetts and North Carolina, seeing compromise as the only way to break the stalemate, provided the margin of victory” (p. 99). By addressing such an important issue in such an ad hoc manner, Jillson raises serious doubts about the explanatory power of his model.

Jillson’s calculation of “who won what” at the convention is even more problematic. Because each state was, at some point, part of a winning coalition, Jillson concludes that the convention accommodated every state’s interests. The arbitrariness of this conclusion, however, is revealed by the fact that one of these coalitions dominated the convention for four weeks at a time when everything was up for grabs, while another lasted for four days when only a few questions of detail remained to be considered. Yet Jillson implies that a state would have benefited equally from membership in either of these coalitions. Because he assumes all issues that generate stable cleavages are important—and equally important at that—Jillson erroneously supposes that being on the winning side of a cleavage is always a significant victory.

In his attempt to discover an objectively verifiable structure in the proceedings, Jillson so abstracts from how and what the convention decided that he overlooks straightforward ways of answering the questions he poses. In assessing “who won” at the convention he could have compared the final draft of the Constitution with both the Virginia and the New Jersey plans. Such a comparison would no doubt have altered his conclusion that the mid-Atlantic states “turned the tables” on the Virginians by the end of the convention (p. 207). To explain why the small states won equal representation in the Senate, he might have looked at the way in which the convention’s decision-making process was structured. He would then have noticed the efficacy of threats of defection in situations where a minority faction can afford to walk out, knowing that it will not be bound by the majority’s decisions and that its departure may well discredit an enterprise whose legitimacy depends on the appearance of unanimity among the political elite. As these examples suggest, *Constitution Making* would have been a much better book if Jillson’s statistical analysis had been informed by an understanding of the political stakes and institutional context of the Philadelphia convention.

American Journal of Sociology

Fiscal Limits of Absolutism: Taxation in Early Seventeenth-Century France. By James B. Collins. Berkeley and Los Angeles: University of California Press, 1988. Pp. xii + 256. \$34.00.

Margaret Levi
University of Washington

If there is a single message in the recent literature on ancien régime France, it is that absolutism was far from absolute. James B. Collins puts one more nail in the coffin of the popular view of immensely powerful pre-Revolutionary monarchs. Through a detailed investigation of the 17th-century taxation system, he demonstrates that the power of the king was highly constrained by the necessity of bowing to local custom and, particularly, to local elites. The result was severe limitations on fiscal power, which made it difficult to extract revenues from significant portions of the population and to ensure that what central government agents did extract reached the monarch.

In *The Fiscal Limits of Absolutism*, Collins argues that the lack of standardization of the French tax system, on the one hand, and the use of tax officials as both sources and collectors of revenue, on the other, had their origins in the inability of the monarchy to break what amounted to contractual relationships with local elites. Moreover, he claims that the apparent economic inefficiencies of such a system were more than balanced by its political efficiencies, at least until the massive reforms of 1634, in which the monarch tried to improve his revenue significantly. The attempt at economic efficiency undermined political efficiency. The perception of broken contracts and increased pressure on the paying capacity of the peasantry combined with resentment toward the use of the army as tax-collection agents to intensify resistance. Increased nonpayment and rebellion in the 1630s and 1640s were the consequence.

The general argument is appealing, especially to those of us who believe, with Richelieu, that "finances are the nerves of the state" (p. 1). The history of its taxation clearly reveals the limits of French absolutism. Indeed, as I have argued elsewhere, English monarchs may have been more powerful than French. Although they could not impose so many taxes, the process of negotiation through Parliament enabled them to collect what they did impose.

The strengths of the book lie in its detailed documentation of where the monies went and who paid. Collins makes a major contribution with his revision of tax figures. He first debunks the figures supplied by Jean-Roland Mallet, the traditional source of French tax scholarship. Then, creatively using the available data culled from tax and budgetary records, he offers a compelling case for the paucity of the revenues that actually reached the monarch.

Collins goes on to posit that the inequities of the system were even greater than scholars have generally recognized. Not only were exemp-

tions rampant among the nobility and clergy, but many of the peasants were not on the rolls. He argues that there were "two societies in rural France: the stable, sedentary society of traditional historiography and an itinerant society of semiproletarians" (p. 184). Thus, the widely recognized pressure on the farming peasantry was even greater than has been supposed.

Collins also provides useful documentation of the extent of non-compliance with the taxation system. That noncompliance should significantly increase after the reforms of 1634, even though taxes themselves did not, fits neatly with his argument that the tax system represented a congruity of interests between the monarch and the nobility up until that period and a divergence after. The monarch hoped to improve revenues through reducing the cut to royal officials. Once tax agents felt that the monarch had broken his side of the "deal," their willingness to comply or to enforce the compliance of others declined.

Collins's account would have benefited from some of the recent literature on rebellion. His is a simple view in which problems of collective action hardly have a place. There are also gaps in his explanations of why lenders continued to cooperate with monarchs who failed to make promised payments. The rational choice literature or the transaction-costs perspective would perhaps have helped him illuminate this paradox.

The research of Collins and other recent historians of ancien régime taxation, such as William Beik (*Absolutism and Society in Seventeenth-Century France* [Cambridge: Cambridge University Press, 1985]), runs parallel with but seemingly without awareness of relatively recent revisionist investigations of the relationship between the French monarchy, nobility, and peasantry. I am thinking of Robert Brenner's articles in *Past and Present*, Philip Hoffman's investigations of the relationship between ground rents and taxes (e.g., "Taxes and Agrarian Life in Early Modern France: Land Sales, 1550–1730," *JEH* 46 [1986]: 37–55), William Bruestein's emphasis on regional modes of production ("Class Conflict and Class Collaboration in Regional Rebellions, 1500–1700," *Theory and Society* 14 [1985]: 445–68), and Hilton Root's documentation of monarchical alliance with the peasantry through reconstruction of the village community by means of the *intendant* system (*Peasants and Kings in Burgundy* [Berkeley and Los Angeles: University of California Press, 1987]). Despite some conflicts of interpretation, all these scholars aim toward the same end. Each set could benefit from the other, and those of us interested in the evolution of the state and of power will undoubtedly benefit from both.

American Journal of Sociology

Sectarian Violence: The Liverpool Experience, 1819–1914. By Frank Neal. Manchester: Manchester University Press, 1938. Pp. xi + 272. \$55.00.

Tony Lane
University of Liverpool

For much of the 19th century, Liverpool was a raw and vital city that, viewed from a distance, frequently seemed on the brink of ungovernability. Regularly, and for three-quarters of a century, the immigrant population of Irish Catholics was locked into violent confrontation with other working-class sections of the population who flew the Protestant flag. These matters provide the substance of Frank Neal's *Sectarian Violence*.

The book is rich in empirical detail, and Neal has scoured the local press and Home Office records to provide a chronological account of sectarian violence from the 1830s to the decade before the First World War. One of the two introductory chapters provides a useful summary of the city's social geography, demography, and general socioeconomic conditions up to midcentury as well as a nuanced synthesis of previous studies of the Orange Order (which cries out, still, for the attentions of a historical anthropologist). The second chapter—"No Popery Politics"—provides useful contextual material on local politics and the Anglican connections before introducing the reader to what the author frequently calls, echoing the prose of late 20th-century popular journalism, "violence on the streets." The following seven chapters are concerned with particular episodes of riot and disturbance, and for each occasion contingent explanations are offered. The Irish famines and enforced emigrations that followed, the local tax burden imposed on Liverpool's ratepayers to aid the starving and largely destitute immigrants, the calculating use of ethnic prejudice for political purpose, paranoid anti-Catholicism, and Irish nationalism are all factors variously adduced to explain the technicolor accounts of combat provided in a local press luxuriating in adjectival melodrama.

The author is unquestionably strong in his faithful transcriptions of local press and constabulary reports. When it comes to explanation, however, the study is embarrassingly naive. The problem, one suspects, lies in the author's intellectual orientations, for Neal is a statistician who practices history as a "hobby." Accordingly, the author has brought to this study all the enthusiasm of the antiquarian and little of the theoretical skill that one associates with the modern social historian who has carefully picked over the offerings of sociology and social anthropology. Indeed, even Neal's knowledge of the relevant historiography is suspect. In a work on riot and disturbance, it is remarkable that the acknowledged masters in this field—George Rudé and Richard Cobb—are never mentioned.

Neal is not wholly blind to the usefulness of conceptual categories. He

Book Reviews

understands that class and occupational status are relevant to positioning the backers and adherents of the Orange Lodges, and he frequently observes the middle-class Tory's distaste at the sectarian habits of their working-class supporters. But class and cognate concepts are used only as descriptive labels and are continually overlaid in the text by the categories of Irish Catholics and Protestants. It is this continual reductionism to religious allegiance and prejudice that makes the implicit analysis so naive.

While the author knows that the social and political dynamics of a rapidly growing city are highly relevant to the events he describes, his categories of Catholic and Protestant are curiously timeless and seamless; they seem always to consist of the same people, who simply respond to external stimuli, be it famine, tax burdens, or ritualism in the Church of England. In reality, Liverpool's working class was mainly differentiated by opportunities in labor and housing markets—religion was a cultural means of defending relative privilege and of engineering solidarity against economic and consequent social uncertainties. The "working class," furthermore, was undergoing constant modification by migrations from the Lancashire hinterland, North Wales, and Scotland, as well as from Ireland. Unfortunately, the social dynamics of the city have been relegated to background in this study, and the result is a book that adds little to the understanding of this British "melting pot." On the other hand, Neal has produced a very good work of reference, and it will be an important starting point for future students.

The Road to Mobocracy: Popular Disorder in New York City, 1763–1834.
By Paul A. Gilje. Chapel Hill: University of North Carolina Press, 1987.
Pp. xviii + 315. \$32.50 (cloth); \$9.95 (paper).

Ted Robert Gurr
University of Colorado, Boulder

The Road to Mobocracy, a social history of rioting in New York City from the Revolutionary era to the Age of Jackson, deftly helps demolish the myth that American society was once insulated from violent conflict by consensual political values. On the evidence assembled here, the civil peace of New York City was disrupted repeatedly by political, ethnic, and religious riots. By my count from Paul Gilje's narrative, more than 70 such riots occurred between 1788 and 1834. While most were minor clashes or mob attacks, more than a dozen were major riots over political and communal issues that involved many hundreds of participants and persisted, some of them for days.

Gilje's thoroughly researched study is more concerned with interpretation than enumeration. The central organizing idea is that mob action underwent a fundamental transformation during the seven decades under

review. Before the Revolution, it was widely regarded, and justified, as an assertion of corporate communalism. It took ritualistic forms designed to assert community values and to ridicule those who violated them, and it rarely involved physical assault. By the 1830s, however, social violence reflected the sharpening divisions along religious, class, and ethnic lines. It often had explicit political content, involved assaults against persons, and was almost universally condemned by civic leaders.

There is a charming, archaic quality to the author's sketch of the rituals of pre-Revolutionary mob behavior. They included the *charivari*, rituals of misrule and role reversal, and the rite of passage. They drew on traditions well established in England, which provided both the occasions (such as *Pope Day*) and the script (maypoles, processions with effigies, parading a victim through town). During the Revolutionary era, these customs were adapted for political purposes. Maypoles gave way to *Liberty Poles*, the scene of many riotous confrontations between civilians and British troops.

After the Revolution, two strands of "politics out of doors" coexisted. Four great riots of communal regulation occurred in 1788–99, directed at doctors who dissected cadavers, at a duplicitous banker, and at bawdy houses, but this kind of defense of common values was in decline as a motive for collective action. In the ascendant from the 1790s to 1814 were riotous processions and attacks involving contending political parties, Federalists versus Republicans and factions within each. Such actions were relatively nonviolent but greatly alarmed local officials and provoked increasingly punitive responses.

Ethnic riots began in 1799 and escalated. While four were initiated by blacks, twice as many were antiblack and typically took the form of mob attacks on black churches. Most common, and most violent, were riots involving the Irish, who were immigrating in increasing numbers and who were more than willing to arm themselves with cudgels and stones and fight back against any who challenged them. The Irish also initiated riots against blacks and played a major role in much of New York City's noncommunal violence: many of the tavern and gang fights and violent disputes among tradesmen and laborers of the 1820s and 1830s apparently involved the Irish.

Predictably, the rising frequency and violence of mob action solidified elite opposition and led to the adoption of what today would be called "law and order" policies. Policing became increasingly professional, and police officers and constables often faced mobs and paid dearly for it, sometimes with their lives. A fully modern and uniformed police force was still decades away, however.

Labor conflict also was relatively common in New York in the half century after the Revolution, but only half-a-dozen strikes seem to have been violent. Gilje expects to find an emerging class basis for collective action. The weight of his own evidence is that communal and political antagonisms were far more important in structuring mob action. He also

Book Reviews

could have inquired more closely into the sources of the Irish disposition to engage in all kinds of violence, from assault and murder to riot. It is reasonable to speculate that the immigrants brought with them a psychocultural disposition to aggression whose origins lie deep in the Irish historical experience.

No general theoretical interpretation for collective action is proposed or used. The study nonetheless is a rich source of evidence that is by and large consistent with theories that emphasize the importance of group identities, value consensus and dissensus, and the significance of traditions and repertoires of collective behavior. There is not much here to support structural or class theories of conflict, or the rational choice models of political action that are currently in fashion.

Policing the Poor in Eighteenth-Century France. By Robert M. Schwartz. Chapel Hill: University of North Carolina Press, 1988. Pp. 321. \$45.00.

Hilton L. Root
University of Pennsylvania

Shaken by the century of chaos, rebellion, and civil war that began with the Religious War and ended with the Fronde, French people in the mid-17th century turned to the central state for the maintenance of public security. Their desire for better law enforcement, Robert Schwartz argues, included a concern for the control of vagrancy and led to an expansion of the state's repressive powers. Schwartz's analysis in *Policing the Poor in Eighteenth-Century France* is based on the registers of internees at the general hospital and the royal *dépôt* in the region of Caen, Normandy. He finds that during the period the state was able to strengthen its "repressive machinery to a greater degree than is generally thought" (p. 10).

The crown's long campaign against mendicity, Schwartz argues, began in the 1660s with a policy of supporting or reinforcing local institutions. One aspect of early policy was putting the poor to work in state-run factories. Colbert, who fathered the scheme, believed that the vigilant supervision of the work force in factories was a means to control vagrancy while increasing French industrial capacity. Colbert tended to attribute poverty to moral laxness, and he believed that the failure of royal enterprises was due to the idleness of the king's subjects. On the failure of a lace factory in Auxerre, for example, Colbert referred to the "prodigious laziness" of the region's inhabitants. Schwartz, however, tells a different story. He believes that workers could have done better on their own. More flexible, more lucrative alternatives existed for those who preferred producing for a private clientele.

Schwartz's study concerns primarily the period after 1724, when royal

American Journal of Sociology

officials decided that national rather than local solutions to the problems of vagrancy were needed. Traditional practices such as the expulsion of beggars by one municipality placed additional burdens on neighboring cities and on the countryside. Coordination and uniform directives from Paris were needed so that the efforts of one city or region did not impose costs on others. For greater coordination, the allocation of state financial aid to subsidize local enforcement was introduced. In carrying out its efforts, the government continued to rely on local hospitals for the internment of paupers and beggars but enlarged the hospitals' repressive functions. The early 1760s marked an important turning point in the expansion of the state's repressive powers when the crown embarked on a nationwide program of internment. At the same time, the practice of interring mostly "dependent poor" (children under 15) was altered in favor of detaining the "delinquent poor" (men capable of earning a living). Female detainees constituted only 25% of identifiable prisoners. Sweeping arrests were especially common during the late 1760s and early 1770s. By the mid-1770s, royal ministers like Turgot argued for narrowing the focus of repression to the truly delinquent and dangerous. Thereafter, the emphasis of repression shifted from beggars toward the more objectionable and threatening conduct of the marginally and wandering poor. Instead of arresting individuals merely for vagrancy, officials needed charges of intimidation, theft, or robbery before confining an individual.

Schwartz's sources reveal that during the 18th century the central state considerably expanded its repressive powers. The kingdom's settled inhabitants came increasingly to look to the state to control vagrancy. Countryfolk were more likely than city dwellers to call upon the *mariéchaussées*, which marked a departure from the 17th century, when agents of the state were viewed with hostility. One is left with the impression that, given the slightest provocation, anxiety over vagrancy could erupt into general panic. Patrols and arrests were needed to keep popular fears of roaming vagrants in check. Thus, Schwartz concludes, the rise of the centralized state produced a "new form of political and psychological dependence, a new cluster of political and psychological expectations" (p. 10). The Great Fear of 1789 makes more sense after reading this book.

Console and Classify: The French Psychiatric Profession in the Nineteenth Century. By Jan Goldstein. Cambridge: Cambridge University Press, 1987. Pp. xiii + 414. \$49.50.

Robert Alun Jones

University of Illinois at Urbana-Champaign

The assault on modern psychiatry begun by Thomas Szasz and R. D. Laing in the 1960s has produced historiographical progeny. Robert Cas-

tel, for example, has argued that psychiatry constituted a response to the legalistic issue posed by the bourgeois revolution—how to justify incarceration in a society committed to contractual notions of legitimacy. In their liberal rejoinder, Marcel Gauchet and Gladys Swain claimed that psychiatry arose instead as a kind of subjective, internalized version of popular sovereignty. And throughout the same period, of course, Michel Foucault subjected psychiatry (as well as other “disciplines”) to a withering, quasi-historical analysis of its role in sustaining the 19th-century liberal state.

It is an essential premise of Jan Goldstein's *Console and Classify* that this “historiography” has been insufficiently historical. The problem has not been simply that its authors have been nonhistorians; it has also been that the policy-oriented controversies in which these works have participated have left important historical questions unanswered. One question, for example, is of the role of French psychiatry in long-term social processes like professionalization, bureaucratization, and secularization. A second is the role of specific historical actors, including bureaucrats and legislators as well as psychiatrists, and the frequently ironic relation between their motives and intentions, on the one hand, and the historical consequences of their actions, on the other. A third is the less secular, prescientific antecedents from which psychiatry slowly emerged.

Goldstein's attempt to answer these questions focuses on three elements of 19th-century psychiatric theory: the so-called moral treatment (*traitement moral*), monomania, and hysteria. The first refers to the cure of insanity by methods that engaged or operated directly on the intellect and emotions, rather than the traditional methods of bleeding and purging applied directly to the lunatic's body. The pivotal work here was Philippe Pinel's *Traité médico-philosophique sur l'aliénation mentale, ou la manie* (1801), which functioned as “the originative psychiatric paradigm” in its systematic explication of the new method. But Pinel's achievement (as he himself recognized) was less one of creating a new method than of legitimating the traditional 18th-century practices of lay concierges, quacks, or “charlatans,” primarily by assimilating these practices to sensationalist psychology, Rousseauean pedagogy, and statistical verification. The paradigmatic status of the *traitement moral* was further enhanced by the crucial Kuhnian criteria: it was sufficiently unprecedented (at least within the circles of official medicine) to attract an enduring group of adherents, and it was sufficiently open-ended to leave many problems for the newly redefined group of practitioners to solve—hence introducing a period of “normal science.” And with the destruction of the medical “corporations,” its articulation occurred within a new form of professional organization—the “patron and circle” structure already familiar to readers of Terry Clark's *Prophets and Patrons* (1973).

The same sensitivity to the interpenetration of social, political, and intellectual history is evident in Goldstein's chapter on monomania. Introduced by J.E.D. Esquirol about 1810, and all but gone by 1870,

American Journal of Sociology

monomania—a single pathological preoccupation in an otherwise sound mind—constitutes an intriguing example of the rise and fall of a disease. In the more fluid and potentially unstable political context that succeeded the Revolution, where “monomania” became synonymous with immoderate and unsettling ambition of any kind, the disease became the vehicle for the governmental advisory role Esquirol proposed for the emerging cadre of “professional” psychiatrists. More successful was its use by Etienne-Jean Georget, Esquirol’s favorite student, as the basis of a broadened insanity defense in criminal cases, one that greatly enhanced the status of “forensic psychiatrists” by integrating their functions into those of the state. Indeed, in this boundary dispute between the medical and legal professions, this partial, elusive form of insanity was the perfect instrument, for it was readily concealable, thus requiring the scrutiny of the trained diagnostician. But the disease began to decline in the early 1850s.

What replaced it? The answer of the Third Republic was clearly hysteria. But hysteria’s Hippocratic origin rendered it one of the oldest diseases in Western medicine. What explains its dramatic increase near the end of the 19th century? Earlier explanations, Goldstein observes, have focused on the patient. But it takes two to make a diagnosis, she adds, turning her attention instead to fin de siècle physicians who, aided by Jean-Martin Charcot’s highly public, positivistic “iconography” of hysteria, were soon approaching every diagnosis with the expectation of hysterical symptoms. In fact, Charcot’s description of the disease was sufficiently publicized that patients, appropriately provoked, could be made to elicit the “correct” symptomatology. Precisely because it was an “intermediate zone” of pathology, migrating downward from the middle to the lower classes, swelling the ranks of the public asylums, and spilling over into outpatient care, hysteria could be “captured” by the newly ascendant science and augment its professional stature accordingly. The power to classify, Goldstein concludes, is indeed one of the most basic, primordial of social powers.

Exhaustively researched, elegantly written, and persuasively argued, *Console and Classify* is an excellent example of the more sociologically informed intellectual history, stimulated by Kuhn and Foucault, that has revitalized that discipline in the past 20 years.

Autocracy, Capitalism, and Revolution in Russia. By Tim McDaniel. Berkeley and Los Angeles: University of California Press, 1987. Pp. xi + 500. \$45.00 (cloth); \$14.95 (paper).

Jeffrey Brooks
University of Minnesota

The author of this ambitious attempt to fit the Bolshevik Revolution into Marxist categories sees Russia as an exception in the history of revolution.

Book Reviews

tions. Russia, Tim McDaniel argues, represents a “very rare historical phenomenon, a victorious revolutionary labor movement” (p. 3). From this vantage point, he challenges Barrington Moore’s powerful paradigm in which peasants loom so large as well as Theda Skocpol’s influential taxonomy.

In *Autocracy, Capitalism, and Revolution in Russia*, McDaniel argues, following Trotsky and, in a sense, Alexander Gerschenkron, that there was a fatal contradiction between autocracy and capitalism. “Autocratic capitalism,” he suggests (pp. 13–35), crippled Russian capitalism, imperiled respect for law and property, disarmed the industrial bourgeoisie, and set labor on a revolutionary course. Russia’s uniqueness, as he sees it, lies in the relationship between the state and labor. Roughly half the book concerns labor policy, and the rest the labor movement itself. Labor policy mattered because the state shaped the labor movement.

The authorities, he suggests, pushed labor into politics. Under such conditions, according to the author’s typology, conscious workers looked to the radical intelligentsia, and the masses to the conscious minority. “Conscious workers” (p. 194) were prevented from engaging in reformist activity, and “mass workers” (p. 175) were “excluded from real participation in society” (p. 169). The result was a revolutionary situation.

One problem with this analysis is that too much is left out. Peasants in revolt weakened the provisional government and helped topple its tsarist predecessor, and the cities were filled with peasants in 1917, many of them women, who came to replace drafted factory workers. Leaving out the nationalities is also problematic. Georgians, Jews, Letts, and others helped make the revolution in the capitals, and their countrymen played a role as well.

Another weakness is inherent in the project. The author trades freely in “mass consciousness” (p. 178), “transformation of values” (p. 190), “will of the proletariat” (p. 395), and “identity” (p. 200). He says for example, “Self-contempt, despair, a sense of meaninglessness: such feelings were very widespread among the mass workers” (p. 173). Maybe this was true, but where is the proof?

The author depends heavily on memoirs. Although he is aware of problems with these sources, he ignores the issue of radical autobiography as a genre. Attention to its conventions would have facilitated a more critical analysis. For example, questioning the prevalence of Sunday schools and other religious activities for workers, the author concludes, “I have seen no estimates of how widespread these institutions were, but as opposed to the night schools, they were hardly mentioned in the memoirs available to me” (p. 170). Why not? Was it that the institutions did not exist, or that the activists did not attend them, or that those who wrote autobiographies did not write about them?

Equally distressing is the lack of quantification. The author does not use slogans on placards or newspaper accounts of labor strife, although

American Journal of Sociology

there is a literature about how to collect "event data" from newspapers, and such materials have been used by labor historians. Moreover, the author ignores the very newspapers workers read, such as the *Kopeck* of St. Petersburg (*Gazeta kopeika*, 1908–18), which was so popular among workers that the Bolsheviks later considered using it as a model for a Soviet mass newspaper. Such publications are absent from the bibliography.

The cavalier attitude toward "mentalities" and a reliance on autobiographies lead to other difficulties. Women seldom wrote such accounts although large numbers of them were workers. Nor did male authors generally write about relations with women, although they presumably interacted with women even during the days of revolution. The author explains at one point that it was "difficult for workers to lead a normal family life" and that many left "spouses behind in the countryside," but there is nothing about those hundreds of thousands of female workers whose husbands, boyfriends, or would-be boyfriends were soldiers or the peasant women who came to the cities during the war to replace drafted workers. The place of women in revolution and in the consciousness of revolutionary men requires some explanation.

Despite these shortcomings, this is a stimulating book with an intriguing interpretation of the Russian Revolution.

Cultural Revolution in China's Schools, May 1966–April 1969. By Julia Kwong. Stanford, Calif.: Hoover Institution Press, 1988. Pp. xix + 200. \$24.95 (cloth); \$16.96 (paper).

Thomas B. Gold
University of California, Berkeley

Scholars and participants continue to debate the causes of China's Great Proletarian Cultural Revolution (CR), which ran from 1966 to either 1969 or 1976, depending on your point of view. Julia Kwong's study of the CR in China's schools reflects the confusion in the field.

She takes the CR as an example of a revolution while acknowledging that this is a debatable proposition. She focuses on the movement's process, not its causes. She traces the way in which the idealistic goals of the students were compromised as workers and peasants replaced intellectuals and students as leaders in the schools, in large part because of the severe infighting among student factions.

Cultural Revolution in China's Schools, May 1966–April 1969, is based on Chinese documents, including Red Guard publications, and interviews of participants at dozens of locations in China and Hong Kong. The book's problems begin to appear on page xi, when Kwong asserts that the CR "was triggered by students' dissatisfaction with the school system." All the evidence brought to light by recent Western schol-

Book Reviews

arship (not cited) and numerous memoirs of Red Guards and others (also not used) clearly demonstrates that the CR originated as a power struggle at the top echelons of the Chinese Communist party (CCP), whose protagonists mobilized, manipulated, and disposed of students and other social forces as the movement evolved. This is not to say that students did not have grievances, but they would not have moved without a push or guarantees from someone above. The book's premise does not withstand scrutiny.

Second, the author does not understand the difference between the CCP and the state. She constantly collapses them as "the government." Understanding at least the analytical separation of party and state is vital to understanding much of the CR as well as the current efforts in China to separate the two in practice. It is also central to any effort—however misguided—to explain the CR as a genuine social revolution.

In the CR, part of the radicals' program was to eliminate a separate state bureaucracy and have the party run all units of the state and society directly. Kwong's conclusion that workers and peasants ("the proletariat") on their own took over schools from the military and administered them as a community project completely overlooks the role of the CCP and the continued role of the military in both the party and society as the only national level organization with any coherence after 1968.

The lack of understanding of the CCP and of the nature of the state it created and ran, as well as of its manipulation of society, dovetails with Kwong's misunderstandings of theories of revolution, such as claiming that "comprehensive theories of revolutions are not available" (p. 151). Throughout this book, after relating some incident, she cites on an ad hoc basis a scholar of revolution such as Theda Skocpol or Barrington Moore as either illustrating her point or being inappropriate for explaining the CR. Because the CR fits no definition of a social revolution and Kwon reduces sophisticated arguments to aphorisms (to wit, "Theda Skocpol has suggested that revolutions occur when the ruling group is ineffective" [p. 11]), she muddles both theory and data. This confirms some of the worst stereotypes held about area-studies specialists.

Speaking of data, in addition to neglecting a great deal of recent writing, the author uncritically incorporates numerous instances of obvious polemic as fact, such as the role of the catchall category "proletariat" in the schools. Some of her interviews corroborate her statements, but she offers no details about how she met her interviewees or how representative any one was of her total sample. In addition, many of her subjects' opinions fly in the face of what virtually all other foreign scholars have found out. There is little new information or insight in the book.

American Journal of Sociology

Rationality and Revolution. Edited by M. Taylor. Cambridge: Cambridge University Press, 1988. Pp. viii + 271. \$37.50.

William Brustein
University of Minnesota

Few phenomena have fascinated social scientists as much as revolution. Research on revolution has generally focuses on why revolutions occur but rarely on why they so seldom occur. In essays that both challenge widely accepted explanations of revolutionary collective action and present a pioneering approach to the study of revolution, *Rationality and Revolution* offers rational choice explanations for why there are so few revolutions.

M. Wallerstein and A. Przeworski explain that workers under capitalism can attain a level of welfare arbitrarily close to welfare under socialism through transfer payments financed by taxes or uninvested profits, so it is not rational for the workers to follow a revolutionary strategy. J. Elster examines Marx's failure to see the capitalist class's behavior—refusing to take government power—in France and England in 1848–52 as rationally motivated. J. E. Roemer discusses Lenin's and the tsar's ideologies to show that revolutions are more likely to occur if a revolutionary's ideological predispositions are also good revolutionary strategy.

The insightful essays by S. L. Popkin, M. Taylor, J. Tong, and C. J. Calhoun tackle the issue of the infrequency of revolution by examining cases of revolutionary success. Each of these authors suggests that revolutions are successful when the "free-rider" problem inhibiting collective action can be overcome. In his essay on peasant movements in Vietnam, Popkin argues that, to overcome free riding, revolutionaries must develop "mechanisms for coordinations of expectations and the pooling of resources" (p. 16). Popkin suggests that, in those regions of Vietnam where political entrepreneurs successfully facilitated conditional cooperation, revolutionary collective action was possible. According to Taylor, to overcome the free-rider problem in revolutionary collective action, a network of local communities must be present. The essential properties of a community are that (a) its members have beliefs and values in common, (b) relations between members are direct and many-sided, and (c) its members practice generalized as well as merely balanced reciprocity. According to Taylor, community facilitates successful revolutionary collective action by making conditional cooperation possible and by ensuring effective social sanctions against noncooperation.

Tong's essay on rebels and bandits during the Ming dynasty, the most satisfying essay in this praiseworthy volume, is the only contribution containing a wealth of empirical data to test a rational choice model of revolutionary collective behavior. Tong finds that the highest proportion of rebellions and banditry occurred in regions where there was a

Book Reviews

minimum likelihood of surviving hardship but a maximum likelihood of survival as a rebel. Tong contends that individuals are more likely to participate in rebellion when they calculate a high probability of success.

In an excellent piece that highlights the dominant role that "reactionary radicals" such as artisans and peasants have played in most modern social revolutions, Calhoun expands Taylor's notion of community. Like Taylor, Calhoun sees community as a means to facilitate conditional cooperation and thereby foster collective participation. But Calhoun notes that free riding is more easily overcome in a community because of the presence of traditional culture.

Popkin, Taylor, Tong, and Calhoun deserve praise for their ambitious efforts to tackle the issue of the logic of revolutionary collective action. Collectively, these four contributors throw a larger explanatory net than they do individually. Popkin's emphasis on the role of political entrepreneurs would benefit by incorporating Taylor's and Calhoun's notions of community. For example, the absence of community might help Popkin explain why revolutionary collective action tended to be sporadic, unorganized, and short-lived in Cochin China but not elsewhere. Tong should find that the probability of success as a crucial factor in explaining revolutionary collective action is significantly influenced by links between individuals and their communities. Both Taylor's and Calhoun's arguments about the link between community and revolution could be strengthened by incorporating Popkin's explanation of political entrepreneurs. Taylor and Calhoun may find that political entrepreneurs are necessary to initiate and facilitate conditional cooperation in communities.

Above all, if one major shortcoming to this otherwise commendable volume exists, it is that the authors, especially Popkin, Taylor, and Calhoun, fail to examine the problem of first-order free riding. They never explain how those institutions responsible for overcoming the free-rider problem have themselves initially overcome the logic against collective action in their own formation.

In sum, this is a rich collection of essays that demonstrate the place of rational choice theory in understanding revolution. What is so appealing about this volume is the application of rational choice theories to a phenomenon that has held center stage in sociological discourse since the mid-19th century.

The Organizational State: Social Choice in National Policy Domains. By Edward O. Laumann and David Knoke. Madison: University of Wisconsin Press, 1987. Pp. xvi + 539. \$45.00 (cloth); \$19.95 (paper).

Charles Tilly
New School for Social Research

In principle, sociologists have known for a long time that they had three possible starting points for general analyses of social behavior: the indi-

American Journal of Sociology

vidual, the society, and the social relationship. In practice, they have commonly planned research as if only the first were possible, theorized as if only the second were conceivable, and acted as if the third were negligible. Structural analysts such as Harrison White and Mark Granovetter have, however, begun to make a difference; sociologists are at last paying increasingly serious attention to social relationships. Edward Laumann and David Knoke have now added weight to the third alternative with a sustained analysis of the effect of relations among organizations on the making of American public policy. Furthermore, instead of explaining the actions of individuals or groups, they have aimed at events—in this case, the production of authoritative decisions. In *The Organizational State*, they have thus begun to close the gap between studies of collective action, in which events have long occupied a prominent position, and analyses of the routine exercise of power.

Singling out national decisions on energy and health, the Laumann-Knoke team interviewed representatives of 333 American organizations that a painstaking survey of relevant sources and experts had shown to be active in those fields; they included congressional committees, federal agencies, associations of state and local governments, research units, labor unions, trade associations, professional societies, business corporations, public interest groups, and lay voluntary associations; the interviews took place in the summer of 1981 and focused on decisions under the Carter administration, which had recently left office. The research group also used a variety of periodicals to identify 166 events concerning the initiation of programs, regulation, or funding. The whole analysis consisted of specifying the characteristics of individual actors and issues, examining the relations among actors and issues, exploring the involvement of different sets of actors in events in health and energy policy, and making preliminary efforts to gauge the effects of organizational action on policy decisions.

Laumann and Knoke spared no technical or conceptual resources; they aimed at redefining sociologists' approaches to their subject. To follow the argument, a reader must know—or be willing to learn—something about multidimensional scaling, about network, hierarchical cluster, logit, factor, and log-linear analyses, about the reading of complex tabular and graphic presentations of results, and much more. He must also acquire a demanding new vocabulary; for example, strings of related events ("event scenarios," in the Laumann-Knoke lexicon) fall into types called the standard decision cycle, the consummated recurrent standard cycle, the unconsummated recurrent standard cycle, and the constructed scenario. Although Laumann and Knoke write serviceable prose, the sheer density of terminology and evidence (not to mention the rarity of interim summaries) makes the book heavy going.

Laumann and Knoke unfold this argument as they go: (1) Organizations' interests in particular issues, their capacities to monitor events involving those issues, and their reputations for influence jointly cause

Book Reviews

their prominence in the networks from which other organizations get information. (2) Interests in issues, reputations for influence, and positions in communication networks jointly cause their positions in networks that exchange crucial resources. (3) Interests in issues, reputations for influence, and positions in resource-exchange networks combine to cause their participation in policy-relevant events. (4) Interest in issues combines with resource positions to create influence over the outcomes of such events. They also argue that (5) events involving similar issues and sequences of actions activate similar sets of actors.

The argument is plausible, if unsurprising, with or without the Parsonian gloss the book gives it. Its most dubious feature is the placement of reputation as a prior variable, when previous performance surely affects reputation strongly. (Indeed, late in the book Laumann and Knoke use their ability to predict reputation as an indirect test of their Coleman-style power-influence model, and in the conclusion they remark that an organization's reputation "is an amalgam of its past successful influence, present strategic location, and future [anticipated] exploits, with a little misperception thrown in" [p. 376].) The evidence generally supports the model. Two significant exceptions appear, however; monitoring capacity repeatedly makes little difference, and position in exchange networks has relatively little effect on participation. It looks as though activist organizations get involved whether or not they have ample resources, but that actors combining large resources and strategic network positions ultimately play the larger part in the actual outcomes—more or less what Washington lore suggests. The analyses portray a health field that divides sharply about means (e.g., federal vs. private care) but not about ends, an energy field riven by dissensus between advocates of governmental and corporate action.

The Organizational State will not resolve debates about the extent to which capitalists run the American state in their own interest, questions about the ability of power holders to keep issues off the national agenda, or doubts about relations between lobbying organizations and the publics they ostensibly represent. But it will set an exacting standard for any future studies of influence over governmental decision making and will make it difficult to assume that the resources and interests of various actors determine their effectiveness in influencing government policy. Its many successes will, I hope, encourage sociologists who begin with social relations rather than individuals or societies to enlarge their ambitions and expand their theoretical scope.

American Journal of Sociology

Reconstructing American Education. By Michael B. Katz. Cambridge, Mass.: Harvard University Press, 1987. Pp. viii + 212. \$22.50.

Kathleen Barry
Pennsylvania State University

In his critical history of American education, Michael Katz sets out to "show how the reconstruction of America's educational past can be used as a framework for thinking about the reconstruction of its present" (p. 1). Confronting the ideologies by which most of American educational history has been written, Katz sharply focuses *Reconstructing American Education*, his study of the origins of public school education, by identifying the social forces that shaped it. He neither reduces schooling to the causal determinants that serve the interests of capitalists nor reifies it into the liberal fulfillment of the American dream. In studying the relationship between schooling and economic and political power, he makes careful distinctions among industrialization, urbanization, and capitalism to identify patterns of social and economic relations that affect education.

Using historical research with social models, the author identifies phases and transformations in the development of the American educational system. He finds that American education was first characterized by paternalistic voluntarism (an early class system), which was displaced by democratic localism evident in the organization of district schools and the corporate voluntarism of early universities and academies. By the mid-19th century, with the advent of the common school movement, school boards, and administrations, the authority pattern developed into incipient bureaucracy. Not only are these models a good fit to the early American educational system because they comprehend the complexity of changes in it, but the historical generalizations they produce are then grounded in a case study of the Boston school system.

By the late 19th century, with the bureaucratization of American education, reformers discovered that the schools were not fulfilling their mission and that teaching was becoming a devalued profession. Katz ably demonstrates that the charges against the educational system in the late 19th century were not different from the complaints that would reemerge in the 1960s—decline in the quality of education, rigidity of the system, and suppression of individuality. But what Katz does not note is that concern over the devaluation of education almost always has structural causes, and in this case it had to do with the recruitment of women teachers to its labor force.

To analyze the interaction effects between gender discrimination and the social valuation of an increasingly female profession, Katz needs more than the perfunctory recognition he gives throughout this volume to the gender and race inequalities that the educational system both produced and replicated. Recruiting women teachers because they could be paid less promoted a white male upward and outward mobility to administra-

Book Reviews

tion and bureaucracy. It was the woman's rights leader and former schoolteacher Susan B. Anthony who framed the problem in an 1852 teachers convention, "Do you not see that so long as society says woman has not brains enough to be a doctor, lawyer or minister, but has plenty to be a teacher, every man of you who condescends to teach, tacitly admits before all Israel and the sun that he has no more brains than a woman?"

Pointing out that educational reform has always held unrealistic expectations of schools, solving both social ills and personal emotional needs, the author successfully uses the history of education to point out directions for recent educational reform. Katz critically reviews how the grandiose expectations of schooling in the 1960s War on Poverty, which were not met, as they inevitably were not, paved the way for racist hereditarian theories of intelligence such as those of Arthur Jensen. The preoccupation with failure led us directly to the conservative educational agenda—the policies stressing excellence of the Reagan administration that are reestablishing a rigid meritocracy to "serve best those who enter them with a favored position" (p. 131). This history is a critical confrontation with those researchers who fail to include analysis of power relations in their educational studies and instead hide behind studies of interdependence of all social phenomena and an "ideology of objectivity."

Reconstructing American Education, which includes the chapter "The Moral Crisis of the University," is an important contribution to the history of education that will provide fresh insights to both the historians and sociologists in this field, yet it is written in a style that makes its critique accessible to students.

Community Organizations: Studies in Resource Mobilization and Exchange. By Carl Milofsky. New York: Oxford University Press, 1988. Pp. xvi + 287. \$49.95.

David Knoke
University of Minnesota

The nonprofit sector of American society employs several hundred thousand people and annually spends tens of billions of dollars. It embraces hospitals, universities, day-care centers, philanthropic foundations, art museums, orchestras, and countless local civic and voluntary associations. Despite its substantial effect on the health, culture, politics, and welfare of the nation, virtually nothing is known about this sector, when compared with the vast army of bureaucrats and academics devoting their lives to monitoring and measuring the activities of for-profit firms and government bureaus. Worse, our factual ignorance about the nonprofits is compounded by a paucity of useful theory designed explicitly for this type of organization, not simply borrowed from other fields.

Hoping to begin filling this vacuum, Carl Milofsky brought together 11

American Journal of Sociology

previously published articles and unpublished papers from the Yale Program on Non-Profit Organizations that specifically examine community organizations. These are "generally small, loosely structured, voluntaristic, and heavily democratic organizations that identify themselves with a specific geographic area of a city, town, or rural area" (p. 3). The selection in *Community Organizations* is a nice balance between discursive essays and high-power quantitative statistical analyses. The topics addressed will appeal both to research scholars and to practitioners. I just wish number crunchers would refrain from using their computer mnemonics when they write up their results.

As is typical of any eclectic collection, the contributors work from numerous divergent theoretical perspectives that tend to speak past rather than pointedly at one another. Susan Rose-Ackerman applies conventional supply-demand economic methods to day-care centers and United Funds and discovers that services are distributed much in the manner an economist would expect. Deborah Polivny's companion analysis of the United Way takes a more political view and finds that admission patterns of new applicants reflect the power of the United Way's two main constituencies: current member agencies and donors. An analysis of Twin Cities corporation contributions by Joseph Galaskiewicz and Barbara Rauschenbach finds support for both quasi-economic exchange theory and the institutional isomorphic approach advocated by Paul DiMaggio and Walter Powell (in their seminal "Iron Cage Revisited" article, reprinted here).

Several authors consider local neighborhood and community organizations of the sort that were fostered by the Johnson administration's War on Poverty and starved under recent Republican administrations. Janet Weiss argues that a rhetorical advocacy of "coordination" of human services persists, despite no evidence of its feasibility or benefits, because of its symbolic suggestion that we can have our cakes of improved services and still eat them at reduced costs. This conundrum is an excellent example of the triumph of myth and ceremony over rationality in organizational politics. J. Miller McPherson's chapter is a reprint of his ecological theory of voluntary organizational niches, tested with data from Nebraska cities. It makes a persuasive case for the priority of sociodemographic structures over attitudinal epiphenomena in explaining the organizational competition for members. The two papers by Milofsky (one with Frank Romo) are less successful in accounting for community organizations' resource-acquisition strategies. However, Albert Hunter and Suzanne Staggenborg use the same questionable data as a takeoff point for speculating about the importance of internal and external networks for resource mobilization and organizational action.

This volume provides a lot of roughage for social theorists to chew on. If we were not previously aware of them, we are alerted to networks, ecology, economic markets, organizational structuralism, political process, and cultural systems. What the book lacks is an effort to sort

Book Reviews

through the welter of competing and supportive paradigms to suggest new directions of theory construction. Milofsky's superficial introduction does little more than tick off a number of encouraging avenues. He was remiss in not contributing a concluding chapter that attempts to integrate these themes into a more comprehensive explanatory scheme. Without that effort, this promising collection remains primarily a testimonial to the current state of intellectual ferment in a long-neglected sector of our political economy.

Structural Equation Modeling with LISREL: Essentials and Advances.
By Leslie A. Hayduk. Baltimore: Johns Hopkins University Press, 1988.
Pp. xvii + 405. \$37.50.

D. Randall Smith
Rutgers University

A modern revolution in model testing and estimation began in the early 1970s with the introduction of Karl Jöreskog and Dag Sörbom's *LISREL* (Linear structural RELATIONS) program. The program is capable of estimating measurement models (e.g., factor analysis), structural equation models (e.g., path analysis), and a combination of both. The sheer flexibility of the program for modeling social science phenomena, coupled with a range of estimation algorithms (five at present, with more to be added), makes any exposition of the procedure a demanding task.

The range of topics covered by *Structural Equation Modeling with LISREL* is ambitious. The first three chapters are reviews of basic statistical theory. Chapter 2 covers single and multiple regression, as well as dummy coding, interaction, and collinearity. In 29 pages, chapter 3 presents the basics of matrix algebra and the calculus of derivatives needed to understand what *LISREL* is doing and how. As Leslie Hayduk notes, this material is presented as a review and not a full treatment of these topics.

The basics of *LISREL* models are encountered in chapter 4, where the eight matrices and three equations defining the models are presented. Chapter 5 demonstrates maximum-likelihood estimation in the *LISREL* context and model identification. Goodness-of-fit indicators, model residuals, and estimated parameters are the subjects of the sixth chapter. Chapter 7 demonstrates how to "trick" the program into providing estimates of interaction terms and nonlinear effects. Interpretation of model coefficients, the decomposition of effects (e.g., direct, indirect, and total effects), and the interpretation of models with feedback loops are covered in chapter 8. The ninth chapter presents both models for multiple groups ("stacked data") and how to incorporate means into a *LISREL* model. The final chapter ("Odds and Endings") covers a mix of topics (ordinal data, pairwise deletion, other estimation strategies, and how to find out why

American Journal of Sociology

you are getting error messages from the program) that were not addressed in other chapters.

Each of these applications of LISREL modeling is demonstrated with data from a survey of households in Edmonton, Alberta, Canada. The substantive focus is the relationship between religious attitudes and behaviors and attitudes toward smoking. The data, control cards, and output needed to replicate Hayduk's examples are provided in the relevant chapters or appendices. These data and examples serve the intended dual purpose of tying together the variety of models that can be estimated by the program and allowing the reader to reproduce these results.

There is much to recommend here. Hayduk is equally at ease explaining the simplest and most advanced applications of the program. Frequent reminders that substantive theory and knowledge of one's data are the keys to successful modeling are given throughout. The bibliography is impressive and will be useful to those wishing to go into any particular topic in more detail. The graphics are quite good and especially useful for illuminating statistical theory and LISREL models.

Some problems do arise in Hayduk's walk through the LISREL maze. Foremost is the strong stand that measurement-error variances for indicator variables be fixed *before* estimating the parameters of a model. This is not a widely accepted practice, and the justification given (pp. 119-23) could be more convincing. The use of multiple indicators for latent concepts, a major conceptual advantage of the LISREL approach, could be given more attention. Most of the latent concepts in the empirical examples have single indicators, and only once (in conjunction with interaction terms in LISREL models) is an example given where a latent concept has more than two indicators. At times the empirical examples of the "smoking" model are strained (as Hayduk readily admits) in an attempt to illustrate the more sophisticated applications of the program; this particular data set is unable to support the full range of LISREL models. At other times, the book's strength of its broad coverage becomes a weakness, as the reader is left wanting more exposition of a given topic (e.g., how to model means within LISREL).

Hayduk has written more than just a solid text for use in advanced graduate courses on statistical modeling. Those with a firm mathematical background who wish to learn about the approach, or those who know a little about the program and want to know more, will find this an excellent reference. Even true "LISRELITES" (Hayduk's term) will find the book useful for its tips on advanced applications and its nicely crafted path through the program's capabilities.

Book Reviews

Analysing Everyday Explanation: A Casebook of Methods. Edited by Charles Antaki. London: Sage Publications, 1988. Pp. 232. £25.00.

Kathy Charmaz
Sonoma State University

The book attempts to provide a comprehensive guide to a range of techniques for analyzing explanations given in everyday talk. Charles Antaki has compiled and organized a set of papers that address the structure and content of everyday explanation, the context of accounts, and rhetoric and ideology. In addition, Antaki and Stephen Draper each offer an initial general framework to set the stage for studying explanations. The contributions are original, with each following the same structure: a description of the author's general approach, his or her specific area of work, a presentation of data to illustrate points, and last, an assessment of the advantages and disadvantages of the method described.

Ironically, despite his intent to cover social science more broadly, Antaki does not draw on Mills, Scott and Lyman, or Hewitt in his interesting analysis of structures of justification. Like many of the chapters in *Analysing Everyday Explanation*, Antaki's provides a brief overview of his argument (that a person's explanation forms a coherent whole and that attributionist reductionism ignores reasoning processes and leaves out context), followed by empirical examples and the development of procedures with which to analyze them.

Some of the chapters are small gems that would make excellent readings for both advanced undergraduates and graduate students. Draper's chapter, "What's Going on in Everyday Explanation?" offers the most thoughtful analysis of explanation that I have come across. In it, he makes the following points: (1) explanation encompasses many kinds of statements, some of which have no connection with causality or reasons, (2) the broadness of the concept and the lack of identifiable relation between type of explanation and surface marker make a mechanical analysis untenable, (3) reasons given may refer to causality, accounts of beliefs, or the purpose of a speech act, and (4) explanations occur within the complex hierarchy of goals involved in any statement and may serve diverse interrelated purposes. Mary M. Gergen offers a lucid analysis of narrative structures in which she argues that accounts are stories revealing coherent narratives. Margaret Wetherell and Jonathan Potter provide a straightforward, readable illustration of discourse analysis.

Throughout the book, tensions emerge between "scientific" inquiry (i.e., methods borrowed or copied from the natural sciences) and interpretative approaches. Some of the contributors remain in the scientific camp and see their task as the impartial application of technique. Others place themselves in the interpretative camp. Some, like Billig, argue for interpretativism but still see method as limited to a set of impersonal rules and procedures for collecting and analyzing data rather than as strategies or points of departure.

American Journal of Sociology

The book purports to cover social scientific analyses of everyday explanations generally and to do so from a variety of perspectives. The categories and content of the book seem broader than they prove to be. Although the book intends to provide a comprehensive exegesis of explanation, the contributors focus on talk and concentrate on related forms of conversational and discourse analysis. The book does show nicely how psychologists are drawing upon a variety of new techniques and approaches as well as reexamining established perspectives such as attribution theory.

Most of the contributors teach in psychology departments in the United States or Britain. Although their contributions no doubt reveal current traditions and emerging arguments in psychology, they do not give the same full consideration to several major approaches in sociology despite their intent to cover social science more broadly. Notably, symbolic interactionist, existential, and phenomenological approaches are not detailed. Although Leudar and Antaki attempt to extend Mead's notion of completion, they do not attend fully to symbolic interactionist assumptions about interpretation. Sociologists have only begun to scratch the surface of explicating *analytic* methods in qualitative work, in general, much less of explicating methods of analyzing everyday explanations.

As a sociologist who remains outside much of the terrain of conversational and discourse analysis, I find the chapters more alike than not, like pieces of the same puzzle. Hence, the range of techniques presented looks less striking and powerful to me than perhaps to someone in these traditions. For that matter, the emphasis on talk alone decreases the possible range of coverage. However, given the narrowness of much of academic psychology, these contributions may seem both innovative and comprehensive to psychologists who seek new paradigms and new techniques.

Sociologists who are interested in analyzing accounts through conversational, narrative, or discourse analysis will want to use the book. Researchers, teachers, and graduate students in these traditions will all find it useful. Sociologists who teach research methods may wish to require students to read selections from the volume to clarify new or unfamiliar perspectives such as deconstructive approaches.

Under Cover: Police Surveillance in America. By Gary T. Marx. Berkeley and Los Angeles: University of California Press, 1988. Pp. xxv + 283. \$25.00.

Lynn Zimmer
State University of New York at Geneseo

Gary Marx admits that he began his inquiry into police undercover tactics with a predisposition against their use. After a thoughtful analysis of their

costs and benefits, he concludes that "given the American context, covert means [are] sometimes the best means" and that the problems associated with their use can be "held to an acceptable level" (p. 206).

A good portion of *Under Cover* outlines the actual and potential problems of covert policing. The most serious is one of crime creation. Criminal informers may use their "cover" to commit other crimes, or they may become "double agents," working for those they investigate. Police agents themselves may be "forced" into committing crime to maintain their covers and prove their criminal intentions. Other crimes are created when targets who may not have otherwise committed a crime are encouraged (or tricked) into doing so by undercover agents. In some cases, public officials who fail to "take the bait" and accept an illegal bribe are later charged with failure to report bribery attempts. When those with no known criminal past are approached in this way, the operation's goal becomes one of determining not whether people are corrupt, but whether they are corruptible. Marx concludes that covert activities should be most suspect when they create rather than detect crimes, although he does not rule out the possibility that certain circumstances may make such tactics acceptable.

Many of the police involved in covert activities also suffer unintended negative consequences. Deception may become a way of life, making it difficult for undercover agents to maintain family relations or return to ordinary policing duties. Agents may, on the one hand, become overzealous in their effort to enforce the law, or, on the other, become overly friendly and sympathetic to those they are watching. Especially in the 1960s, when there was considerable covert surveillance of leftist political activities, many agents became converts to the groups' causes. Excessive stress and psychological trauma also occur. In one undercover unit of 70 officers, Marx reports that 52 were eventually indicted on criminal charges, two committed suicide, and one had a mental breakdown.

There are negative consequences for society as well. Because there is the potential to use covert tactics against people for their political beliefs, such tactics may have a chilling effect on political dissent. Their use may also serve to erode trust among members of society, especially when the police pose as doctors, lawyers, priests, and journalists. These tactics also offer some risk of convicting the innocent because the evidence often produces an illusion of certainty. In fact, all the technology making this kind of surveillance possible can also be used to create "fake" evidence; covert activities magnify the potential for corruption and misuse of authority that regular policing offers.

There are, nonetheless, some benefits to the use of undercover tactics. The most compelling one offered by Marx is that they can be used to uncover crimes of the elite that cannot be easily detected by traditional policing strategies. High-level crimes in business and finance; political corruption among government officials; fraud committed by doctors and lawyers—these are crimes that regular police forces are powerless to

American Journal of Sociology

detect and ill equipped to investigate. Over the past few decades, many criminologists have revealed the property loss and human injury caused by elite crime and have complained about the class bias inherent in crime enforcement. Marx convincingly argues that only undercover tactics and covert surveillance can remedy this bias and offer some protection to a public that has become increasingly concerned with white-collar crime.

In the final chapter, Marx presents an overview of the technologies of the new surveillance: computers and data bases, miniature video and audio recorders, satellite photography, polygraphics, and biotechnology. These technologies, used not only by the police but also by employers, intrude into private lives and have the potential to turn everyone into suspects and our culture into "a maximum-security society." In spite of these dangers, Marx favors the use of these technologies by the police as long as they are subject to control. He suggests warrant requirements, review boards, monetary compensation for innocent targets, and clearer guidelines for the entrapment defense. Marx also suggests that such tactics should be used only as a last resort. He does not, however, identify the specific circumstances under which they ought to be allowed and ought to be limited. He hints at, but falls short of saying, for example, that covert activities should be used primarily against crime by the elite—white-collar and political crimes that cannot be uncovered through other techniques. This is where the strongest case can be made since these are the criminals who might use the new technologies to commit and cover up their crimes. If undercover tactics are widely used against "street crimes," the class bias built into current enforcement patterns will remain.

This book is oriented more toward raising questions than providing answers. It does a good job of outlining the issues and identifying the means-ends dilemmas inherent in the use of police covert activities and undercover surveillance techniques. In the end, Marx suggests that we may have to choose between anarchy and repression in accepting or rejecting them. Let us hope it will never come to that.

The Social Psychology of Procedural Justice. By E. Allan Lind and Tom R. Tyler. New York: Plenum Press, 1988. Pp. vii + 267. \$32.50.

Henry A. Walker
University of Iowa

The study of distributive justice emerged as one of the most fertile topics of scholarship in social psychology during the 1960s and 1970s. The publication of Thibaut and Walker's *Procedural Justice* (Hillsdale, N.J.: Erlbaum, 1975) and the "discovery" that individuals evaluated the justice of procedures in addition to the justice of outcomes suggested an even more fruitful future. It seemed clear that (as Thibaut and Walker put it) whatever procedural justice was, it was unlikely that distributive justice would be achieved or perceived without it.

The purpose of E. Allan Lind and Tom R. Tyler's *Social Psychology of Procedural Justice* is to "present a picture of the advances in the study of procedural justice" (p. 203) since the appearance of Thibaut and Walker's book. The authors fulfill that promise by providing a systematic survey of the literature on procedural justice. The book should be of interest to new students of the phenomenon and to those who need a review of the most recent research on the subject.

Lind and Tyler provide a useful discussion of the early foundations of procedural justice research, including the initial findings of Thibaut and Walker (chap. 2). The core of the book (chaps. 4-8) includes systematic summaries of research that confirms and supplements Thibaut and Walker's early work on procedural justice in the law and on legal institutions (chaps. 4, 5). In subsequent chapters, these findings are extended to politics and government (chap. 7) and to organizations in the public and private sectors (chap. 8), thereby establishing the generality of procedural justice phenomena.

The authors draw a number of conclusions from their review in chapter 9. Some of the conclusions are empirical generalizations; for example, "Procedures are viewed as fairer when they vest process control or voice in those affected by a decision" (p. 208). However, a significant number of them are not and could simply be called observations; for example, "Procedural justice effects are robust across methodologies" (p. 206). In addition, the authors propose several hypotheses that presumably should be investigated empirically.

The authors state that they have not attempted to construct a "unified theory of procedural justice" (p. 221). Instead, in chapter 10 they describe two models of procedural justice phenomena (the self-interest and group-motive models), generate predictions from the two models, and compare the predictions with existing findings. Lind and Tyler conclude that neither model fits all the data well and suggest how the two models may be reconciled.

Critical readers may be less than satisfied with this offering. First, many of the tables fail to report levels of statistical significance or *N*'s, and the reader is left with the authors' interpretations of the findings. Second, an examination of questionnaire items utilized in the assessment of fairness (app., pp. 243-47) indicates that many of those items employ terms that might be treated more correctly as correlates than as indicators of fairness or justice. The issue of appropriate indicators points to an unresolved issue in procedural justice research. There is no generally agreed on definition of a just procedure. The absence of such a criterion suggests important methodological questions about the respondents' referents for evaluations of procedural justice and the extent to which findings are comparable across investigations. This issue, not discussed by the authors, is associated with a general lack of critical analysis in the book.

Lind and Tyler should not be singled out for failing to formulate a criterion of procedural justice or for failing to investigate the various

American Journal of Sociology

alternatives. However, a more critical stance would have brought such issues to the reader's attention, enhanced the usefulness of the authors' review, and might possibly have paved the way for further advances in the study of procedural justice.

Juvenile Correctional Reform: Two Decades of Policy and Procedural Change. By Edmund F. McGarrell. Albany: State University of New York Press, 1988. Pp. xvii + 219. \$34.50 (cloth); \$10.95 (paper).

Mark D. Jacobs
George Mason University

The cycles of juvenile correctional reform are of broad sociological interest because (to borrow Albert Hirschman's phrase) they mark the "shifting involvements" of American history, the general redistributions of public and private responsibilities. Only since the 1960s has the federal government acknowledged any role for itself in juvenile corrections, but most responsibility for juvenile correction remains with the states. In recent decades, California, Massachusetts, and Washington have taken perhaps the most influential initiatives. The persistence of significant variations in correctional policy among the states creates a need for such studies as the case study of New York in *Juvenile Correctional Reform*, to facilitate interstate comparisons.

Edmund F. McGarrell attempts to replicate the Massachusetts case study made by Alden Miller, Lloyd Ohlin, and Robert Coates (*A Theory of Social Reform* [Cambridge, Mass.: Ballinger, 1977]). Drawing primarily on semistructured interviews with "key participants" in correctional policy formation (identified through a snowball sampling procedure), as well as on archival data, McGarrell assesses the validity for the New York experience of the five empirical principles identified by Miller and colleagues as explaining correctional policy reform. He traces the interplay among liberal and conservative interest coalitions and formal decision-making groups to explain legal and policy changes during four successive directorships of New York State's Division for Youth. He reports trends in annual data produced by correctional agencies to indicate the effects of those changes.

The "Edelman years" from 1975 to 1979 were pivotal. Initially successful in implementing his deinstitutionalization policy and having been selected as architect of the Juvenile Justice Reform Act of 1976, which retained family court jurisdiction over even serious juvenile offenders, Peter Edelman lost control of correctional policy when Governor Carey (responding to a well-publicized incident of violent juvenile crime) suddenly switched positions in the heat of his 1978 reelection campaign. The Republican Senate, Democratic Assembly, and Democratic governor quickly agreed on the Juvenile Offender Act of 1978, transferring original

jurisdiction over juveniles accused of specific serious crimes to the adult criminal court. The political defeat represented by the passage of that act contributed to Edelman's resignation soon thereafter, leaving his successors to rebuild the juvenile system's capacity for secure confinement.

The chapter on the Edelman years foreshadows one of the most interesting findings, that "the structure of state-level decisionmaking tended to vary between an administrative mode and a politician mode" (p. 172). What is problematic, then, is the process of "agenda setting" that determines the movement between these alternating modes as well as the corresponding prospects for fundamental policy change. This finding is consistent with the empirical principle suggested by Miller et al. that receives the greatest support from McGarrell's analysis—the principle of "sequencing," that is, that change is most likely to occur when there is a congruence between internal, aspired to choices and external, available ones.

The study has many strengths. Its longitudinal focus extends over two decades, covering swings of the policy pendulum in both directions. The annotated sociograms provided by McGarrell to diagram the interplay of actors in each act of the formal policy drama help to clarify his account of each policy shift. The limited application of McCarthy and Zald's "political economy" model of social movements, which usefully complements the principle of sequencing, aids McGarrell in exploring the proximate forces operating on every policy decision in a thorough and systematic manner.

Yet because McGarrell limits himself to the exploration of *proximate* forces, I have reservations concerning the scope of this study. Although he proposes to address, as his key question, "*How and why* has juvenile justice, particularly juvenile corrections, changed during the last twenty years?" (p. xi), the empirical core of this book reads like a series of quasi-official legislative histories interspersed with sections from a correctional agency's annual reports. A theory of social reform needs to be cast in a more critical spirit and on a grander scale. Although McGarrell alludes to the importance of larger structural forces, he does not integrate them into his analysis. The brief historiographical survey of American juvenile justice in the first chapter and the consideration of "broader issues" in the concluding six pages of the book are disembodied appendages to the case materials.

McGarrell altogether ignores such factors as demographic shifts in the age structure of the population resulting from the baby boom or the potential for private profit from corrections in an economy losing its industrial base. He introduces only incidentally such other factors as fiscal crises, crises of prison overcrowding, state-federal relations, the diffusion of policy innovations across state boundaries, and developments in related policy areas.

In sum, this carefully crafted case study serves a needed purpose and is useful as far as it goes, although its methodological focus is too narrow for its theoretical frame.

American Journal of Sociology

Social Epistemology. By Steve Fuller. Bloomington: Indiana University Press, 1988. Pp. xv + 316. \$27.50.

Steven Yearley
Queen's University of Belfast

Steve Fuller remarks early in *Social Epistemology* (p. 4) that the term could easily be regarded as an oxymoron. Epistemology, a normative discipline, has concerned itself with the nature of and grounds for human knowledge. In line with these concerns, rationalist philosophers of science have sought to establish the basis for the special success of scientific knowledge and to outline methods or procedures that will guarantee the optimal growth of knowledge. For its part, the sociology of knowledge has sought to show how knowledge is socially determined or shaped; its ambitions have been descriptive and explanatory, telling us how—factually—knowledge has come to be the way it is. Within science studies these two positions have, in the past two decades, struggled for supremacy.

Fuller's objective is to outline a plausible social epistemology, a non-banal accommodation. His general arguments for this are set out in the opening two chapters, in which he defines the business of social epistemology. He proposes that (in part, at least) epistemology is already deeply empirical, and, correspondingly, that sociology can be normative. Thus, 19th-century epistemologists foreshadowed this endeavor insofar as they occupied themselves with the relationships among scientific disciplines. Arguments about which scientific discipline was the most fundamental and about which science's claims should have preeminent authority already constituted a philosophical inquiry with practical implications. Epistemological disputes were already empirical in the sense that they focused on "the range of options . . . provided by the actual social history of knowledge production" (p. xi). Indeed, Fuller goes as far as to claim that "epistemology has been a well-motivated, autonomous field of inquiry *only inssofar* as it has been concerned with the social organization of knowledge" (p. 5; my italics).

Now, defined in this way, epistemology is clearly a project with which social analysts of science can engage. In particular, the relationships among disciplines and the erecting and maintenance of boundaries around specialisms have been leading topics in the sociology of science. And sociological attention to these issues can lead directly to questions of policy. For example, with the increasingly complex division of labor in science, how are claims from neighboring areas to be evaluated when one cannot test them oneself? What are the best ways to organize interdisciplinary research? As Fuller comments, these kinds of policy (and therefore normative) questions stem "from empirical considerations on the social nature of knowledge growth which have been deliberately avoided by" contributors to much recent social studies of science (pp. 269–70).

The sociology of science is thus offered the chance to participate in "naturalistic epistemology" (p. xi). Observations on the way that the social organization of knowledge affects knowledge production can be developed into an applied science for research policy. In other words, the social epistemologist is "the ideal epistemic policy maker" (p. 3).

In seeking to justify these claims, Fuller divides the book into four parts: the introduction to social epistemology already described, a section on the philosophy of science, a section on the sociology of science that deals principally with the nature of scientific disciplines and their boundaries, and two concluding chapters on research policy.

Sociologists of science are most likely to benefit from the last two sections—in particular, the discussion of the nature and growth of disciplines and of the social processes whereby the characteristics of science are ascribed and its demarcation from "nonsense" is drawn. Fuller's outline account of the developmental history of disciplines (see pp. 193 ff.) is incisive and witty. He makes the point that disciplinary growth generates areas of ignorance; it promotes systematic noninvestigation as well as the deep study of certain issues. And it is such observations that are taken up in the closing policy-oriented chapters. He develops the arguments of, among others, "finalization" theorists to suggest ways in which policy interventions can direct the extension of knowledge, not just by focusing scientists on one topic rather than another but by changing disciplinary relations and challenging areas of systematic ignorance.

Fuller's book persuades me that social epistemology is indeed a possible and absorbing project, but he does appear to leave a number of issues untouched. This may be partly a matter of style; the book ends abruptly without drawing the issues together in an inviting way. But some issues also seem to be considered too lightly. For example, science is increasingly assessed in utilitarian and technological terms. What is the social epistemologist to make of this, especially since the connection between science and technical knowledge is still so poorly understood? Can the social epistemologist advise only on the growth of science per se or on the growth of useful science as well? Second, what can the social epistemologist say about the ideological uses of scientific knowledge? Finally, even if social epistemology is on the cards, does that mean that sociologists and philosophers will stop disagreeing? Less quarrelsome practitioners may opt to collaborate as social epistemologists, but others will continue to argue over the influence of social factors on the development of scientific knowledge. Fuller may have provided an optional alternative, but he has not removed the potential for dispute.

American Journal of Sociology

Explaining Science: A Cognitive Approach. By Ronald N. Giere.
Chicago: University of Chicago Press, 1988. Pp. xxi + 321. \$34.95.

Adele E. Clarke
Stanford University

Philosophers argue. *Explaining Science* is Ronald M. Giere's argument for a new cognitive approach in science studies. Compared with most philosophy of science, the book is extraordinarily accessible. Early chapters offer a coherent history of the philosophy of science that should be helpful to students and other newcomers to interdisciplinary science studies. They both introduce Giere's own perspective and provide a feel for how the philosophy of science is done.

The volume centers on the debate in science studies between realists and relativist social constructionists, which has been very lively since Kuhn's work was published in 1962. Giere has set himself the task of developing a realist approach that fully addresses key issues raised by constructionists (e.g., contingency and negotiation), abandons the tedious legacy of logical empiricist philosophy, and refutes relativism. He calls his approach a naturalistic, scientific, constructivist realism. He poses an evolutionary model of science grounded in natural cognitive mechanisms and assumes the cognitive and social are separable. The sociobiological idea of "epigenetic rules" for making scientific inferences may be helpful, Giere asserts, in capturing the "deeper structures" of science: "Cognitive processes are to the development of science as genetic mechanism are to the evolutionary development of populations" (p. 18).

Focusing first on representations (models and theories), Giere offers a theory of theories, linking scientific models with the "schemata" of cognitive science and emphasizing similarities between abstract models and real systems. He then demonstrates his approach in a laboratory study of a nuclear physics research facility. Here he fully accepts the contingent and negotiated character of research. He avoids other constructionist conclusions by asserting that since scientists' language usage is clearly realist, those who study scientific work should use that same language and conceptualization.

Turning to scientific judgment, Giere reviews research on whether humans are probabilistic information processors (Bayesian agents) and finds such claims unsupported. He argues instead for an account of scientific decision making based on the notion of "satisficing" (doing the best one can with what one has), drawn from bounded-rationality theory developed by Herbert Simon in organization studies. Scientists' uses of satisficing strategies are illustrated in a strong chapter on experimental design and data handling in nuclear physics. The concluding chapter fully applies his cognitive approach to the "revolution" in geological theory in the 1960s. This is less successful because the analysis seems underdeveloped.

As a constructionist, I find that my main difficulty with the volume is not that Giere disagrees with or refutes constructionism. The problem is more fundamental; Giere seems to understand neither the social nor the construction of reality. I was reminded of W. I. Thomas's old dictum that if people believe something is real, it is real in its consequences. Giere by and large does not see social phenomena as real in their consequences. Only nature is real, and, for Giere, social phenomena do not occur in nature: "The real issue . . . is the extent to which, and by what means, nature constrains scientific theorizing" (p. 56). Social constructionism may account for social reality (and there is only one reality), but it cannot account for scientific reality.

For Giere, social phenomena and social constructions are houses of cards—epiphenomenal, mythic, and ephemeral. They do not engender hardy, durable perspectives that concretely structure and texture how people do things together.

Giere's experiences as a physicist before he turned philosopher have led him in two directions. First, he seeks to address the nitty-gritty, hands-on aspects of actual scientific practice. He accepts scientific work as negotiated and contingent because this is transparent to him as routine aspects of daily work. To these he adds the idea of satisficing, which I like very much, and which demonstrates how science studies can benefit from organization theory and research.

It is the second direction that poses difficulties. Giere essentially advocates a scientist-identified science studies: if scientists believe something is real, we should too (p. 124). Giere wants science to be successful but never specifies the goals of success beyond that of "good" science. On several grounds, I find this a dangerous stance. First, it would resituate science studies in some new-wave version of the classic internalist perspective of examining how scientists, step by valiant step, draw back the curtain to reveal nature in her splendor. Second, there should be room in science studies for varied science criticism. Third, a profession-centered speciality is limited. For example, "medical sociology" has only recently begun to study how people conceive and deal with health and illness in all the situations where they occur—medical or not. Science studies needs similar breadth.

Despite our differences, I am grateful that Giere, unlike many philosophers of science, takes both sociology and social constructionism seriously. I applaud and share his interdisciplinary stance and look forward to continuing the vital dialogue.

American Journal of Sociology

Narrative Knowing and the Human Sciences. By Donald E. Polkinghorne. Albany: State University of New York Press, 1988. Pp. xi + 232. \$44.50 (cloth); \$14.95 (paper).

Robert J. Richards
University of Chicago

Donald Polkinghorne believes that our lives are like players who strut and fret their hour upon the stage. He proposes that we understand our destiny by attending, not to the stars or to the code bred in the bone, but to the plot that gathers up our scattered actions and makes them significant. It is a tale that is told and that is to be comprehended scientifically in narrative, and we have been speaking it all along.

In *Narrative Knowing and the Human Sciences*, Polkinghorne, a professor of counseling and a practicing psychotherapist, maintains that "human beings exist in three realms—the material realm, the organic realm, and the realm of meaning" (p. 183). This last is the domain of the human sciences, and developments in several of them suggest that the keys to understanding are furnished by narrative. In a preliminary chapter, he defines "narrative" as a story relating a series of events, either true or false. Narrative construction and comprehension correspond, he asserts, to one of two kinds of human rationality—"narrative rationality," which "understands synoptically the meaning of a whole, seeing it as a dialectic integration of its parts" (p. 35), or the other kind, which uses formal logic and mathematics and dominates the sciences of the material and organic realms. In three subsequent chapters, Polkinghorne, in summarizing the work of several theorists in history, literature, and psychology, intends to provide models for the other human sciences, models of the way narrative meaning both produces and explains human action. It is the philosophers of history—especially Ricoeur—who furnish Polkinghorne with the elements of his own conception of how narrative ought to function as the fundamental instrument of the human sciences.

In the current philosophical dispute about the nature of historical explanation, Polkinghorne sides with those who believe narrative accounts have a unique explanatory power. In contrast, Carl Hempel and other logical empiricists argue that every science explains events by showing that they are governed by general laws. Hempel maintains that narratives in history explain events only to the extent they make appeal to the requisite laws and antecedent causes. Polkinghorne agrees that the covering law model serves the natural and biological sciences, but he thinks it fails to capture the meaning of human action. In his view, history and the other human sciences require a kind of narrative logic, which essentially has two aspects: first-order sentences that refer to events that "have actually happened in the way reported in the sentences of the narratives" (p. 62) and a second-order synoptic coherence among the statements, that is, a configuration in a plot structure (p. 63). It is the plot structure that

displays the human experiences told about, especially their temporal dimensions.

Polkinghorne proposes that a person's own narrative understanding of his or her life causes the behavior expressive of that intimate story; therefore, in his view, scientists must learn to read people as they would a text: "Acting is like writing a story, and the understanding of action is like arriving at an interpretation of a story" (p. 142). It would be a mistake, he thinks, to try to explain human behavior by using general laws, whether these be physical, biological, psychological, or social laws, since "bodily movement is 'caused' by the meaning to be expressed" (p. 142).

Polkinghorne's book captures the enthusiasm for narrative that has recently animated much discussion in the philosophy of history and in literary theory. And he has done a decent job of summarizing the various views expressed in these areas, though, for the uninitiated, the descriptions may seem a little vague. However, there are several problems in Polkinghorne's account that may cause anyone working in empirical science or the philosophy of science hesitancy about his conclusions.

First, a vagueness envelops too many of his assertions about the nature of narrative knowledge, for instance, when he proclaims that the human sciences "do not produce knowledge that leads to the prediction and control of human experience; they produce, instead, knowledge that deepens and enlarges the understanding of human existence" (p. 159). Since the Enlightenment, the criteria of science, that is, "knowledge that deepens and enlarges the understanding," have been prediction and conceptual control through the application of general principles. It is incumbent on anyone attempting to discover another kind of rationality to show that it is not merely the complexity of situations and poverty of appropriate laws that distinguish the human from the natural sciences. Although the antecedents of a human act may never exactly reoccur, this itself does not imply that the meaning of "narrative cause" is different from "cause in formal science," as Polkinghorne seems to think (p. 173)—else we must abandon the death of the dinosaurs or the formation of our solar system to storytellers outside the pale of "formal science." Surely, other things being equal, we accept as plausible a narrative history that conforms to relevant, well-confirmed physical, biological, or psychological principles and reject as implausible a history that violates such principles.

Polkinghorne's own analysis of narrative, while making some interesting points (especially about its temporal dimensions), lacks the resources to establish necessary distinctions in applying narrative to the explanation of human actions. So, for instance, when he maintains that we construct our own behavior much as a writer formulates a narrative text—an interesting idea with some potential—he goes little further than reiterating the proposal. He never attempts to distinguish, for instance, the several basic ways in which meaning is expressed in narrative. In our personal narratives, when the author is simultaneously the actor, it is not easy to see how various kinds of meaning might be comparably expressed

American Journal of Sociology

or be the "narrative cause" of behavior. Shakespeare, it seems, intended the actions of the porter in *Macbeth* to provide emotional relief before another round of heinous murder, but the character himself intended no more than to respond to the knocking as if he were the merry keeper of the devil's door. If we are gatekeepers spinning out our actions along a narrative line, we may hear the knocking, but not recognize the purpose of the imaginary door.

Feminist Theory and the Philosophies of Man. By Andrea Nye. New York: Croom Helm, 1988. Pp. x + 244. \$49.95.

Sondra Farganis
New School for Social Research

Feminist Theory and the Philosophies of Man is a succinct summary of the ways in which feminist theory encounters and does battle with liberalism, Marxism, psychology, and structuralism. Andrea Nye finds that these theories allow for dialogue with men but do not significantly advance feminist objectives. Can women use these theories without entrapment? Working over these theories helps correct the theories, but a feminist perspective requires building on women's experiences. By using as her prologue Ovid's tale of a weaving contest between Athena and Arachne, Nye sets the frame for her interpretive reading of feminist theory. Some women use the text and symbols of male argumentation, only to be bested by those who draw on female experience to write their stories. Yet, those who weave in women's terms are, while finer craftsmen or women, divorced from power.

Liberal democratic theory and socialist critiques of capitalism do not explain gender inequity, nor have the social movements undertaken in their name alleviated the plight of oppressed women. Equal rights, equality of opportunity, and the right of participation and consent were denied women when they could not vote. While liberals of the Humean or Rousseauistic stripe, and even women like Madame de Staël, called attention to women's differences (natural and social) from men, democratic theory per se allowed for a new conception of persons. By emphasizing the social and legal nature of relationships and by formulating very general principles of natural and equal rights, liberalism lighted the way for equitable treatment of those not yet included in the fraternal bond. The lead was taken by Mary Wollstonecraft, Harriet Taylor, and John Stuart Mill.

Legal equality—freedom from (overt) discrimination—has not assured women genuine, equitable social and economic treatment. Such treatment requires constant state intervention, which, Nye argues, runs counter to liberal democratic theory. Moreover, such theory rests on ideas of individual rights that are often in conflict with each other, as in abortion and pornography disputes (woman vs. fetus, right to view pornog-

rathy vs. right not to be depicted in a discriminatory way). Nye alludes to, and I would emphasize, how heated these debates become if we introduce ideas of class and of women who are poor, for whom the sale of their bodies become a way of earning a living (as, e.g., in the surrogate-parent controversies).

If socialists were, in theory, prepared to argue for genuine and meaningful equal treatment for women, in practice they were unable to put such policies into effect. Marxism, most especially in its critique of the bourgeois family, emphasized the sociality of one's being and precluded discussions of any "natural" way of being-in-the-world. Women's treatment stemmed from historically constituted capitalist arrangements. The question of a redefined, nonbourgeois sexuality is one from which traditional (male) Marxists shy away, in part because of their concentration on those questions that seem to be central to production—and reproductive issues are not. Contemporary feminists, building on Marxist notions, will reposition production/reproduction arguments and call for assessing sexual and reproductive contributions as productive labor.

Simone de Beauvoir initiated a discussion of women against this background of failed liberal and Marxist theories, as well as at a moment in time—after World War II—when ideas of reason and progress were called into question. Existentialism rejected a disembodied body capable of abstract reasoning, preferring to start with embodiment, with a person in time, a constituted subject whose very consciousness was given clarity by intersubjectivity, that is, by the gaze, glance, acknowledgment, or recognition of another or others. Like those feminist writings that rework Freud (Juliet Mitchell, Jane Flax, Nancy Chodorow), de Beauvoir's contribution is to see the self as an essentially social self, not as a self outside history and outside concrete familial relationships in which women and men play certain specific roles.

The French feminists (Luce Irigaray, Helene Cixous, Julia Kristeva), building on notions of difference rooted in bodily constructions, move to emphasize the importance of language and text in writing on and about gender. In the spirit of Jacques Lacan and Jacques Derrida, they talk about the links between words and oppression, but Nye does not make the connection between their epistemology and their politics, a connection she makes quite well with respect to both liberalism and Marxism.

The important sociological question is whether, indeed, the philosophies Nye discusses are as androcentric as she suggests; if they are—and I am not yet ready to concede that—then feminist contributions will either have to be reworked to make them applicable to women or be abandoned completely. The excitement of the field of feminist social theory is in the prospect of one or the other's being done.

American Journal of Sociology

Max Weber: Essays in Reconstruction. By Wilhelm Hennis. Translated by Keith Tribe. London: Allen & Unwin, 1988. Pp. 254. \$39.95.

Braulio Muñoz
Swarthmore College

Max Weber: Essays in Reconstruction consists of five separate essays related by a central question: How is Max Weber to be read today? The volume shows the strengths and weaknesses of most collections of essays published as a book. On the one hand, the kinds of issues Wilhelm Hennis addresses and the style of his writing—the Nietzschean turns of phrase, the sense of play, the pregnant statements given full reign—fit rather nicely within the confined parameters of the essay form. On the other, the narrative often moves in cryptic circles, relying heavily on the reader's erudite knowledge of 19th-century social philosophy in general and social theory in particular. Consequently, the essays appear to be aimed at a small circle of academics whose main business is to be subtle about matters concerning Weberian scholarship.

And yet Hennis argues that there are fundamental reasons why the proper understanding of Max Weber's life and work should be important to all those concerned with modern social theory and research. The proper understanding of Weber's lifetime work, he argues, would have the effect of making us "aware of the questions that we no longer pose, and thereby provide an impulse for the posing of new questions" (p. 104). To appreciate Hennis's effort, it is necessary to follow the thrust of some of his specific claims. Because of space constraints, I shall note only three such claims.

One of the most controversial claims Hennis wishes to make is that sociologists have smothered Weber's vision by appropriating him as the founder of sociology. From Reinhard Bendix to Talcott Parsons, sociologists have presented Weber as *the* founder of a science that tries to shake free from grounding values and from the great Western conversation concerning the "good life" and the "good society." Rather than being a man preoccupied with founding sociology, Hennis argues, Max Weber was at home with the moral science of Karl Knies (economics) or with the moral teachings of Nietzsche. Insofar as Weber approached the study of society scientifically, he should be seen as a political theorist in the tradition of Machiavelli, Rousseau, and Tocqueville.

In his rush to rescue Weber from the clutches of sociologists, Hennis does not ask himself why Weber would be seen as *the* founder of sociology. Surely, sociologists have claimed other founding fathers. Nonetheless, Hennis has made an important point. Many sociologists today do pretend to see their work as taking place within the "objective" parameters of science. It is a sad commentary on the state of sociology that its initial grounding values—given in the work of Weber to be sure, but also in those of Marx, Durkheim, and Freud—are no longer seen as constitutive of its practice.

Book Reviews

A second point Hennis is intent on making is that, contrary to the claims of such scholars as Friedrich Tembruck and Reinhard Bendix, Weber's central question was never "What is the meaning of rationality?" Rather, Weber's guiding question was connected with the development of humanity (*Menschentum*), with "the fate of man as man" (p. 46). To appreciate properly the "inner sense" of Weber's work, argues Hennis, one has to be prepared to reconstruct Weber's vision from the scattered available pieces; one has to be prepared to grasp Weber's unconscious motivations (p. 65). Above all, one must avoid the artificial division, so current today, between the young and the old Weber. Weber's youthful report on the Polish peasants, for example, must be seen as deeply connected with his late work on the "life spheres," where the disenchantment of the Western world is displayed with courage and daring.

Hennis shows a disciplined command of the materials at hand to make his case. His training as a lawyer (he betrays a sense of pride in noting that Weber, too, was trained in the law) serves him well. But there is an important and unexplored contradiction in this reading of Weber's work. How could Weber be fundamentally concerned with the fate of humankind while holding on to the nation as the only pole on which modern man could center his nihilistic existence?

Finally, Hennis claims that contrary to much recent talk, even by such subtle critics as Leo Strauss and Robert Eden, Weber cannot be considered either an old or a modern liberal. Weber never believed in the liberal theory of progress; he never held on to the liberal idea that an increasing harmony of individual interests will eventually produce happiness; he never believed that mechanisms of checks and balances will preserve, much less produce, freedom. At most, argues Hennis, Weber might be seen as holding forth a "liberal voluntarism" (p. 186), where the Nietzschean contempt for those last men who invented happiness would be kept at the level of consciousness. More than anything else, he argues, Weber's work must be seen as exemplifying the "logic of judgement," that knack for gathering disparate facts into a vision even, or precisely, when the facts point to a reality that is falling apart.

The Causes of Progress: Culture, Authority and Change. By Emmanuel Todd. Translated by Richard Boulind. New York: Basil Blackwell, 1987. Pp. xv + 217. \$34.95.

Harvey J. Graff
University of Texas at Dallas

The Causes of Progress is a bold little book. Its author, Emmanuel Todd, declares that it is "one step on the way towards a new interpretation of historical change," in its emphasis on the role of "stable anthropological factors" in the "modernization process" (p. xiii). For Todd, it is family structures that, in their influence on ideology and now on cultural devel-

opment, constitute those "stable" factors, whereas development is recast culturally and constituted—theoretically and empirically—in terms of rising rates of literacy. The relationships asserted to connect the two, family structure—especially in terms of authority and status relations—and consequent literacy levels become, in Todd's formulation, the "new" engine of human change, past, present, *and* future, and the centerpiece of his attempt at a challenging revisionism.

In *The Causes of Progress* Todd has a heady agenda that sometimes outstrips its abilities to explicate and persuade. In accents and emphases unmistakably French, he seeks to establish general laws of worldwide development, achieve predictive capability, integrate the long sweep of historical and comparative cross-cultural dimensions, and reorient theory and empirical practice away from "economistic" views of development and change. Todd seeks instead to direct attention toward "the concept of progress." Finding political economy "indifferent," he searches in the interstices of the familial structure—literacy level linkages to (re)discover the human engine of progressive transformation.

With an unblushing idealism, Todd conceptualizes human progress as that of the "human mind," symbolized and measured by rates of literacy. Cultural development itself—"the spread within society of one basic intellectual technique: the ability to read and write"—is no less than the "modernization of the mind." It constitutes a "cultural revolution [that] demands to be considered an independent social phenomenon, preceding economic development in time" and "not simply the product of material wealth nor simply of improvement in the conditions of existence" (pp. 1–3).

Literacy, also taken to represent "the learning process," is shaped by family structure, receiving its greatest stimulus from "authoritarian" (in terms of parent-child relations, deemed "vertical") and "feminist" (in terms of husband-wife relations, deemed "bilateral") tendencies in familial structures. More moderate stimuli to cultural development derive from "egalitarian" and "absolute nuclear" family structures. A high average age at marriage, with an associated longer period for "the learning process," contributes to the connections. Both from the spread of literacy from family structure, in turn and in time, comes decline in mortality and fertility. Only later do material transformations follow. Thus, the classical approach to development, which takes England as paradigmatic, is replaced by the "cultural," with Germany, Scandinavia, and Scotland as the new historical leaders, and Japan and Korea today's exemplary cases.

With simple models and simple correlations, Emmanuel Todd sets out to recast history and social science scientifically, in theory and practice. He claims there is great novelty in his effort, on occasion far more than he should. Sometimes brilliant in conception and flash of insight, often refreshing, typically brash and tendentious, Todd's effort is also very frustrating and its rhetoric far too loose and imprecise.

Todd's major contribution derives at least as much from the stimulus

Book Reviews

his conceptualizations provide as from the achievement itself. Many questions and doubts mark any critical response to the enterprise's conception, execution, interpretation, or epistemology. Despite his important effort to frame approach and conception historically, Todd largely ignores much recent historical writing that often relates to, but severely complicates and sometimes outright contradicts, his effort. This is true of his major cases: Scotland, where Rab Houston's exceptional work qualifying the extent and significance of literacy is not mentioned despite its availability; Germany, where work by Gerald Strauss and Bob Scribner is ignored; Sweden, where the work of Egil Johansson is not mentioned; and a much larger roster. The findings of the historical research preclude generalization in the form of the simple-factor, direct, acontextual, and unmediated relationships in which Todd's conception is rooted. Moreover, the essential role of specific historical contexts for understanding human uses and meanings of literacy militates strongly against the kind of abstract, universalizing conception of literacy that governs *The Causes of Progress*. It is more than a bit ironic that recent scholarship, historical and other, severely restricts the Enlightenment-propagated equation of literacy with progress.

In these and related issues, Todd's book raises serious questions about his practice of historical social science and the epistemology and methodology on which it rests. His numerical data are often noncomparable and also lack meaning outside their contexts. Simple representations of rates of literacy, however carefully constituted and criticized, cannot bear the weight of "progress," "civilization," "democracy" that *The Causes of Progress* places on them. These complications, as well as the very modes of analysis and explanation themselves, not only contain an unpersuasive approach to very difficult questions of causal relationship over time and space but also risk the dangers of circular reasoning, simplistic overdetermination, and anachronistic employment of historical method, evidence, and conceptualization. The "cause of progress" does not receive its due here.

War Memorials as Political Landscape: The American Experience and Beyond. By James M. Mayo. New York: Praeger Press, 1988. Pp. xvi + 306. \$42.95.

Barry Schwartz
University of Georgia

Few things affirm the unity of a nation more dramatically than the memory of its wars and war dead. Accordingly, when Abraham Lincoln pleaded for reconciliation in his first inaugural address, it was natural for him to expect that "the mystic chords of memory, stretching from every battlefield and patriot grave to every living heart and hearthstone, all

American Journal of Sociology

over this broad land, will yet swell the chorus of Union. . . ." In hindsight, however, some of these chords of memory are sweeter sounding, some battlefields more revered, some patriot graves more tenderly preserved than others. Describing and accounting for this variety of America's commemorative symbolism are the purpose of James Mayo's *War Memorials as Political Landscape*.

Proceeding more from his early training in architecture and urban planning than from his later training in sociology, Mayo provides all three fields with an important sourcebook. *War Memorials* does not successfully situate its contents in theoretical models of society and culture, but it is an expertly written and usefully illustrated book. It contains information and commentary not only on a vast array of conventional war memorials but also on monuments that mark the least noble sides of war: massacres, death camps, and genocide. The objects that fix these events and sites in the collective memory include "sacred media," like cemeteries, battlefields, and shrines, and "profane media," like private collections of militaria, leftover equipment and war remains, and commercial museums and tours. All these objects, as described in Mayo's first two chapters, bear an identical function: the symbolic depiction of war as the ultimate political act.

The main points of reference for war as a political act are whether it is won or lost and whether it is supported or opposed on moral and practical grounds. Mayo does not use these distinctions systematically, but they show up throughout his book. These two dimensions, outcome and justification, have corresponding, definite patterns of memorial genre. The first pattern commemorates wars deemed by everyone to have been just in motive and victorious in outcome. In America, only three such wars have been fought—the American Revolution, World War I, and World War II—and their commemoration, for Mayo, sets the standard against which all other war remembrances must be measured (chap. 3). Independence Hall, Valley Forge, the Washington Monument, the stunning World War I memorial complexes in Indianapolis and Kansas City, the Arlington Tomb for World War I's unknown soldier, the Iwo Jima monument outside Washington—these devices are the most majestic that Americans have ever constructed in honor of their past.

In contrast to America's "good wars" are those that were less broadly supported at the time they were fought and won, or that were called into question by later generations. Mayo treats of these wars and their memorials in "Monuments to Victory as Manifest Destiny" (chap. 4). Ambivalence over such wars, he shows, is expressed in the memorials themselves, which "are often less visible than those for America's unquestionably just wars." While the more questionable wars "are remembered, individual battles and heroes are often commemorated more than the fact that a war was won" (p. 118). Thus, the War of 1812 is commemorated mainly by the "Star Spangled Banner," a symbol so powerful that it conceals that war's every negative aspect. The Indian Wars, too, have been rarely

Book Reviews

commemorated, and the markings that do exist are often obscure, like the sign pointing to the site in Colorado of the "Sand Creek Battle or Massacre," suggesting that no one really knows for sure what happened there.

"Monuments to Defeat" (chap. 5) is the last chapter devoted expressly to war memorials, and here Mayo is at his worst. He confounds noble causes pursued and lost with stalemates over causes to which the people were indifferent. He assigns no separate category to unmitigated defeat after long and hated struggle. Behind this taxonomic confusion lie factual errors. Attributing to the 19th-century South a "dilemma of remembrance," Mayo wrongfully embraces a current point of view. To those who fought for the Confederacy and to their children, the lost cause was in truth a noble one, and the memorials they built to honor it differ little from those built to honor the cause of the North. Bad wars, on the other hand, warrant special attention, and in discussing them Mayo is on firmer ground. When the cause of a war is widely held to be immoral or, at best, needless, then "defeat . . . cannot be forgotten and a nation's people must find ways to redeem those who died for their country to make defeat honorable. This can be done by honoring the individuals who fought rather than the country's lost cause" (p. 170). Thus, the most prominent memorial to the Vietnam War, the Vietnam Veterans Memorial in Washington, D.C., is by name as well as by design dedicated not to a cause but to the men who pursued it.

Contrasts in the perception of different wars are thus aligned to differences in the volume and quality of commemorative media. The correspondence, although strong, is imperfect, for the design and production of monuments are shaped by more than the way wars are perceived. Mayo explains that the golden years of monument building were between 1890 and 1930. The Spanish-American War and World War I were fought during this period but were not in themselves responsible for its intense commemorative activity. Wealth produced by the industrial revolution, along with the incentive of the City Beautiful movement, inclined Americans to catch up with their past by building monuments to all previous wars, especially the Civil War, as well as to the wars they had fought themselves. World War II, on the other hand, may have been America's best, but it produced relatively few traditional memorials. The golden years had so saturated the landscape with such forms that America chose to mark the great war with unprecedented numbers of "utilitarian monuments," such as hospitals, schools, civic auditoriums, and coliseums.

James Mayo has done an admirable job of describing both the media and the messages of war commemoration, and students of culture would profit from reading him. To get the most out of Mayo's book, however, these students must bring along their own interpretive framework. Mayo's initial analytic scheme, which is based on a fourfold classification of memorials according to the oppositions of sacred and profane and utilitarian and nonutilitarian, really does not sustain the analysis; it peters

American Journal of Sociology

out early in the book, whereupon another more implicit scheme takes over. This second mode classifies wars rather than monuments, and the criteria are whether the wars have been won or lost, whether they have been regarded as morally justified or unjustified. Mayo never bothers to bring his two schemes together. The taxonomies provide frames of description rather than analysis and therefore add little to general understandings of secular symbolism. Readers may well come to their own understandings on this matter, but whether they do so by drawing on the classics of Lloyd Warner (confused in this book with Sam Bass Warner) and Maurice Halbwachs or on more recent models of semiotic and cultural analysis, the added light will not diminish the credibility of Mayo's central belief: that the war memorial is a *via regia* to the understanding of a nation's conception of itself, its ideals, and its past.

Freak Show: Presenting Human Oddities for Amusement and Profit. By Robert Bogdan. Chicago: University of Chicago Press, 1988. Pp. xiii + 322. \$29.95.

Steven C. Dubin

State University of New York at Purchase

Anatomy is not destiny; packaging is. So argues Robert Bogdan in his imaginative study of biological anomalies and fabricated human attractions. In *Freak Show*, his social constructionist position firmly stakes a sociological claim: "It is not necessary that one be a born freak to have a brilliant career as one" (p. 249). Within a single framework, the author considers not only people incorporated into the sideshow because of congenital conditions (e.g., Siamese twins) but also those who were brought there merely because they were foreign ("Ubangis"); those who altered their appearances or developed unusual skills (tattooed people or sword swallowers); and gaffed (fraudulent) representatives of any of these categories. In all cases reputation and monetary success derived from effective presentation.

Many freaks satisfied public curiosity by offering provisional explanations of otherwise mysterious phenomena. This was popular science for the price of admission. And, as we know from previous investigations of the production of cultural forms, generally there are well-structured *worlds* to be discovered behind such activities. Bogdan thoroughly charts the institutional nexus that supported the display of freaks from approximately 1840 to 1940. He examines the different venues where these performances were mounted, elaborates the occupational culture enmeshing freaks and their managers that richly flowered into a unique worldview, and explores how that culture adapted to changing public interests in social, political, and medical issues.

Bogdan also differentiates two modes of presentation, the exotic and

Book Reviews

the aggrandized. One emphasized difference and inferiority by accentuating the primitive. The other elevated the status of the individual, either by conferring honorific titles (e.g., "General" Tom Thumb, the midget), or by juxtaposing conventionality and extraordinary features. Bogdan displays his formidable skills by analyzing countless examples in light of this distinction. A pair of promotional photographs points up its usefulness: the same black giant was presented as both an exotic Zulu and a dignified military figure (p. 113). This aptly illustrates his contention that "freak" is a way of thinking, of presenting, a set of practices, an institution—not a characteristic of an individual. Freak shows can teach us not to confuse the role a person plays with "who that person really is" (p. 10). Armed with his insights, we are not surprised to learn that some individuals appearing in "native villages" at the 1893 Chicago World's Fair were in fact recruited from local pool halls.

Just as the freak show was shaped by large social forces, it began to decline when important ideas were reformulated after the turn of the century. Scientific discoveries clarified the etiology of many physical conditions, for example, and medicalized them through a new vocabulary. Similarly, increased exploration of the world began to undercut common notions about strange "races" of people and their equally alien customs. But Bogdan also provides a fascinating analysis of how new opportunities for display developed as others diminished. His discussion of such self-made freaks as tattooed people, snake charmers, and wild men highlights the elasticity of supply and demand in this market and demonstrates how innovations could rapidly become passé.

This work will inevitably be compared with Leslie Fiedler's *Freaks* (New York: Simon and Schuster, 1978). Although Bogdan criticizes that work for being "mythological" (p. 7) and less comprehensive than his own study, the two books can be considered complementary. Bogdan excels in detailing the *how* of this salesmanship, but he underplays the question of *why* these products were so appealing. Fiedler is more explicitly concerned with physical and symbolic boundaries and the ways in which freaks address our insecurities regarding scale, sexuality, individuality, human nature, and life and death. Bogdan obviously recognizes the evocative nature of this material because his book contains 79 illustrations, a remarkable characteristic for a serious social-scientific study. But while their inclusion tacitly acknowledges the uniqueness of this world, the author of this fine work mostly concentrates on puncturing its aura.

American Journal of Sociology

The Culture of Capital: Art, Power and the Nineteenth-Century Middle Class. Edited by Janet Wolff and John Seed. Manchester: Manchester University Press, 1988. Pp. 236. \$60.00.

David Brain
Indiana University

At first, the essays in *The Culture of Capital* seem to address only a limited audience. The editors, Janet Wolff and John Seed, locate their project in relation to debates in British historiography about the "nature and specific identity" of the middle classes in certain northern industrial cities in the 19th century. In fact, the book addresses larger questions in ways of interest to anyone studying the relationship between culture and class formation.

In the lead essay, Simon Gunn criticizes the notion that the British bourgeoisie somehow failed in its historical role. On the basis of a reexamination of the historical evidence, Gunn dismisses the claim that the middle class was split between northern industrial and southern commercial interests, and that this led to political dominance by the landed classes. The economic and occupational structure of the 19th-century middle class was, he argues, far too complex to be reduced to such a dichotomy. Furthermore, there is no reason to think that economic divisions necessarily produce significant cleavages. Gunn also dismisses the view that a "counterrevolution of values" drew the middle class into the cultural hegemony of the landed classes and thus produced a failure of both entrepreneurial energy and political will. He calls for careful attention to the "internal structuring of the middle class itself" (p. 31), to its actual cultural practices, and to the specific network of institutions in which the middle class was organized in the Victorian city.

The subsequent chapters help inter the notion of bourgeois "failure" and add historical flesh to an alternative view. John Seed exorcises the stereotype of the northern middle class as uncultured philistines with his detailed look at art patronage in Manchester, 1775–1860. Voluntary associations such as the Royal Manchester Institution (1823) are seen not as expressions of preexisting class interests and orientations, but as the means by which the middle class was "constructed as an active social agent" and the "lower orders" integrated into the cultural hegemony of capital (p. 66). Caroline Arscott's detailed examination of the Polytechnic Exhibitions in Leeds (1839–45) shows that, while different interests were represented, the exhibitions provided a context where cultural representations could constitute a common terrain of cross-party collaboration in a generalized rhetoric of "the civilising virtues of art and science" (p. 150). They were one site on which orderly representations of class relations were constructed.

Three of the essays focus more directly on the relationship between cultural representation and social structures. Of particular note is

Book Reviews

Caroline Arscott's interpretation of W. H. Hunt's *Awakening Conscience*, a painting commissioned by Thomas Fairbairn in 1853. Hunt produced a controversial depiction of a "fallen" woman starting up from her lover's lap with an expression of sudden moral anguish. Fairbairn's hesitant acceptance of the completed painting leads Arscott to try to explain the patron's doubts and to reconstruct the reading of the painting that might have finally allowed him to accept it. She examines the painting in the context of other paintings that Fairbairn bought or commissioned from Hunt before and after the one in question. The play of themes and visual elements among paintings suggests a reading of this one as a moral lesson in support of the middle-class ideology of domesticity. Arscott uses a patron's hesitation and eventual acceptance of the painting, Ruskin's positive assessment of its merits, the critics' rejection of it, and the public's "incomprehension" to position the painting in the broader culture and to draw out meanings that could have been operating in that context.

Arscott's psychoanalytic interpretation of the voyeuristic pleasure and anxiety potentially incited by the painting may be overly speculative, but her analysis of the range of reactions provoked by the painting permits a revealing account of the painting as a "problematic image in the 1850s." Her ingenious interpretive strategy illuminates the deep interplay of pictorial conventions and ideologies of class, gender, and domesticity in the constitution of middle-class culture.

The final chapter, by Arscott and Griselda Pollock, with Janet Wolff, analyzes visual representation of the northern industrial cities in paintings, watercolors, and prints. Although some have argued that artistic conventions simply inhibited depictions of urban and industrial settings, these authors focus on the "manipulations and manoeuvrings within and between these conventions" necessary to represent "modern life." Artists were willing to take up urban and industrial themes, but they faced a problem of accommodating pictorial conventions to the representation of ideologically problematic subject matter. The analysis focuses on the interaction between inherited modes of representation and the effort to make sense of the new modes of life and relationships that went with the industrial city.

Throughout the book, the authors argue that there was no homogeneous middle class but that a sense of a "middle-class" identity and a coherent capacity for action were constructed through specific cultural practices in a range of institutional sites. In the tradition of E. P. Thompson, "class" is specified historically and in terms of cultural practices and institutions that integrated (but did not dissolve) distinct and often conflicting interests. The notion of a "coherent class identity" of the middle classes remains analytically elusive, however. One is left wondering exactly what it means to use the language of class when neither economic interests nor conflicts with other classes are at the center of the analysis, but this failing is not peculiar to this book.

Culture and Consumption: New Approaches to the Symbolic Character of Consumer Goods and Activities. By Grant McCracken. Bloomington: Indiana University Press, 1988. Pp. xv + 174. \$27.50.

William Leiss
Simon Fraser University

Described by its author as a collection of essays (some previously published), *Culture and Consumption* avoids the disjointed character of many such collections because of Grant McCracken's care in providing an initial, very useful overview of the whole and his stitching together of the individual pieces with numerous internal cross references and connecting passages.

Nothing indicates the author's command of his subject matter better than the story he tells in a footnote on page 145. A "University of Chicago professor" (an economist?) characterized his choice of a Volvo car as one made for purely utilitarian reasons and resisted McCracken's suggestion that there were symbolic forces at work in this consumer decision. McCracken then made him an offer: McCracken would purchase, insure, and maintain another vehicle on his behalf, bringing his car transportation cost down to nothing. The only condition was that, in the car, McCracken would include certain purely superficial design features, including "fur lining for the seats and dashboard, a hood ornament that showed a rampant horse, and dice for the rearview mirror." The professor declined the offer with the comment that the decorative touches made the car "less useful" to him.

I hope that the whimsical character of this anecdote will not put off a humorless reader. For this is a superb book, a definitive exploration of its subject that makes use of the full range of available literature. Its central theme is the charged interaction between the domains of culture and consumption in the modern West, beginning with the 16th century. On the one hand, consumption (the creation, purchase, and use of consumer goods) "is shaped, driven, and constrained at every point by cultural considerations"; on the other, "culture is profoundly connected to and dependent on consumption."

Thus stated, the thesis may appear trivial. What makes it not is, first, the range of McCracken's conceptual sweep, which pulls in the very broadest meanings that may be attached to these two domains and, second, his exquisite attention to detail, which illuminates the nooks and crannies of everyday life in what are called "consumer societies." The result gives the reader a refreshing new appreciation of otherwise rather shopworn concepts.

The book is organized in three sections. The first contains an examination of historical themes (such as "patina") in the early modern period. The second is called "Theory" and presents the focal point for the surrounding investigations of empirical detail. Drawing on an extensive literature in anthropology, history, and sociology, McCracken synthesizes

his sources on the subject of "meaning transfer" as it works in and through the world of goods or objects. His three dimensions of meaning construction (summarized on p. 72) are "the culturally constituted world," which includes advertising and the fashion industry, "consumer goods," and the "individual consumer"; and he surveys the broad range of meaning-transfer devices, such as differentiation, encoding and decoding, and rituals, that operate across these three dimensions.

Part 3 is entitled "Practice" and deals with the themes already surveyed in a more analytical manner. Here some approaches that are well known in the literature—such as the "trickle down theory" stemming from Simmel and the so-called Diderot effect—are reconsidered in the light of the most recent literature.

McCracken concludes by summarizing the cultural "functions" of goods in our society. Culture is embodied in goods, and this "making concrete" of abstract representations brings cultural traditions to life in the everyday world. Goods also incorporate a tension between a stabilizing effect (giving an order to meanings) and an impetus to change and creation.

The "ratchet effect," where consumption patterns appear to spiral ever upward (in terms of the relative goods intensity of time, to borrow Staffan Linder's expression), and the "displacement effect," where goods stand for the effort to recover lost meanings ("Rosebud" in *Citizen Kane*), are two categories explored by McCracken that raise critical questions for an evaluation of the cultural functions of goods. McCracken rushes across this territory with unseemly haste, depriving us of the insights he could have bestowed on these topics had he pursued them in greater detail.

Instead, he refers in passing (p. 115) to the creation of a society "in which tastes and preferences have internal limits, in which a sufficiency of goods becomes a consumer reality," and continues, "Without these limits, without this sufficiency, there can be no reapportionment of resources within Western economies nor between the economies of the first and third worlds." Here we do not even have his usual brief references to goods sources, much less an adequate exposition of these provocative ideas. This will not do. His readers should demand redress in his future writings.

Becoming an EX: The Process of Role Exit. By Helen Rose Fuchs Ebaugh. Chicago: University of Chicago Press, 1988. Pp. xvii + 247. \$27.50 (cloth); \$10.95 (paper).

Lucinda F. San Giovanni
Seton Hall University

One of the most fascinating features of modern society is the departure by individuals from roles that are central to their social lives and personal identities. In contrast with earlier times, when individuals lived out their

American Journal of Sociology

lives in one marriage, one occupation, one community, or one religion, significant shifts in such factors as cultural values, institutional imperatives, structural opportunities, ideology, and psychological orientation have produced the unprecedented and richly textured phenomenon of our times known as role exit.

Until recently, sociologists have worked to expand their understanding of role entrance and have made substantial contributions in the voluminous literature on socialization. It is only in the past decade that both academic and clinical professionals have shifted their attention to the other side of role change, that of role exit. In this provocative and ambitious monograph, Helen Ebaugh's goal is to extend the sociological interest in this topic by developing a middle-range theory of role exit, which she views as a generic social process similar in theoretical importance to such other processes as socialization and conflict. Her aim is to construct a conceptual map of role exit that identifies salient concepts and testable hypotheses about the conditions under which role exit occurs. Another central concern of *Becoming an EX* is to differentiate socialization and role disengagement from the role-exit process itself, with the first two being viewed as variable aspects of the larger dynamic of role exit. A final guiding interest for Ebaugh is the forging of a clearer conceptual link between the social aspects of roles and their psychological interplay with self-identity.

The theoretical orientation that guides the collection of data and their analysis is an eclectic one. Ebaugh uses the perspective of role analysis, creatively combining the insights of both structuralists and interactionists. She is committed to addressing the subjective meanings that persons bring to their role exits and uses grounded theory and life-history approaches to do so. Specifically, Ebaugh makes careful and critical use of a dynamic stage model of role exit: the patterned, predictable stages through which people pass on their way out of central roles and identities.

Having spent a decade researching role exits, Ebaugh selects a wide range of substantive cases for comparative analysis. These cases are organized into three general types: exits from occupational roles, exits from major family roles, and exits from stigmatized roles. Using a snowball sampling technique, she selected four samples of respondents for a total of 185 cases. With the assistance of several of her graduate students and using taped, intensive interviews, Ebaugh gathered her data concerning primarily voluntary, rather than forced, exits and exits from roles vital to self-identity. A profile of the sample reveals a serious limitation in the general applicability of the theory: 49% of the sample had a total yearly family income of \$40,000 or more, 61% had at least a college degree, and 62% of the sample were men. In effect, this is a sociological portrayal of the journey taken by the more privileged, powerful members of our society and therefore must be read with appropriate caution.

The book is organized in a natural-history fashion, with each chapter devoted to a major stage in the role-exit process. Using the properties of

Book Reviews

status passage first codified by B. Glaser and A. Strauss (*Status Passage* [New York: Aldine, 1971]) and those derived from her analysis, Ebaugh examines 11 variables that shape the nature and consequences of role exit. Woven into her story is information on how these properties (e.g., voluntariness and centrality of the role, degrees of control, institutionalization, and awareness) shape the motives, choices, relations, and feelings that attend leave-taking.

Sociologists working in this area will be challenged to extend and qualify Ebaugh's model in several important respects. First, her theory fails to specify how major social variables, such as gender, race, social class, and age, influence role exit. Second, we must incorporate into the model the findings (mostly neglected in this study) from the basic literature on retirement, the "coming out" of gays, the dynamics of social mobility, and other important exits that characterize the contemporary social landscape. Finally, greater use of the insights from the emergent field of the sociology of emotions would help highlight the links between structural and personal dimensions of role exit.

In spite of these and other limitations, this work belongs in the book-case of every sociologist. Our colleagues in applied sociology will also learn much from reading it. Last, this work offers something special for a broad segment of the population who will be making these journeys themselves in the near future.

New Wine in Old Wineskins: Evangelicals and Liberals in a Small-Town Church. By R. Stephen Warner. Berkeley and Los Angeles: University of California Press, 1988. Pp. xii + 355. \$37.00.

Nancy T. Ammerman
Emory University

In the late 1970s, when Stephen Warner arrived on the scene, Mendocino Presbyterian Church, like many other churches in America, was undergoing an evangelical renewal. During the 1950s, the small, stately, mainline church in this northern California community had been a center of civic life, a club to which good citizens belonged. As an institution, it thrived. As the 1960s arrived, so did activism. From fair housing to the war in Vietnam, activist pastors led the church to protest injustice. While the Mendocino church did not experience great conflict over these causes, its institutional life waned.

In the midst of that activist era, the church began a mission to the hippies of the counterculture living all around them. That mission grew into a charismatic fellowship, and the leaders of the fellowship gained enough influence in the church to guide the selection of an evangelical graduate of Fuller Theological Seminary as the next pastor. What happened over the next several years, as the 1970s faded into the 1980s, was a

American Journal of Sociology

resurgence of church membership and participation. Through sheer institutional strength, the congregation was giving more to its denomination's liberal causes than they had in their protest days.

But what also happened was conflict between the free-wheeling charismatic fellowship and a church concerned with both this world and the next. The story of that conflict, along with the stories of earlier eras in church life, are told in this book with the gripping realism of a well-written novel.

But it is more than a good story. In remarkable ways, this one congregation parallels the odyssey of American Protestantism over this period, and Warner has skillfully interwoven the particular and the general to make comprehensible the ups and downs too often buried under grand theory or pious polemic. He shows convincingly how one congregation is shaped by large cultural forces and peculiar local history, by personality, and by theology. In fact, if there is a level of analysis missing, it is an examination of the intermediate organizational links between local congregation and larger culture. An account of these links to schools and seminar leaders, mission causes and magazines, would have added enormously to this story of American religion.

Warner is able to do what he does, however, because he is utterly faithful to the case at hand while grounding that case securely in a theory of religious expression that reaches beyond a single instance. His careful gathering and analyzing of data is matched by an equally careful theoretical development of their meaning. Warner's data included documents and interviews, along with his own participant observation. He reports on his findings in narrative, in quotations, and in charts and graphs. His methodologically sophisticated work should be read and emulated by every sociology student.

The theory he constructs contrasts two structural forms, nascent and institutional, and two ideological contents, liberal and evangelical. He shows how the resulting four combination types explicate both the life of Mendocino Presbyterian Church and the cycles of American Protestantism. He also shows how these types bring some coherence to many other attempts to categorize religious expressions.

The conduct of this study, its analysis, and the way it is described make it an exemplary sociological work. What makes it truly important, however, is its subject matter. Warner tackled the subject of evangelicalism when it was barely more than a curiosity in academic circles. By leaving his own ivory tower, he discovered the vitality and appeal of a movement that had refused to go away, despite repeated academic pronouncements to the contrary. He shows us evangelicals who are ordinary people, *modern* people, members of a mainline church, concerned about *this* world, yet convinced that the boundaries of this world are permeable to the influences of another. Warner's book documents how the evangelical heritage in America has survived and adapted, taking the materials of modern culture and infusing them with sacred meaning. The reality

Book Reviews

brought to life in these pages is likely to continue to haunt those who theorize that modernity inevitably erodes evangelical faith.

Tales of the Field: On Writing Ethnography. By John Van Maanen. Chicago: University of Chicago Press, 1988. Pp. xvi + 173. \$25.00 (cloth); \$7.95 (paper).

Dennis Ray Wheaton
University of Chicago

John Van Maanen here gives a spirited, self-reflexive guide to the rhetorical styles used in "the cultural representation trade," a quirky performance-art Strunk and White for ethnographers and their readers. *Tales of the Field* analyzes the authorial voices and literary devices used by ethnographers in describing and, increasingly, interpreting the social reality of others. Van Maanen spins and weaves his own insights and recent work by Howard Becker, James Clifford, Clifford Geertz, Joseph Gusfield, George Marcus, and George Stocking to fashion an important response to Becker's call in *Writing for Social Scientists* for "a thorough analysis of the major voices in which academics and intellectuals write" (1986, p. 37). Van Maanen's clever presentation—the book is stuffed with puns and bons mots—suggests that harmonics are better than voices at capturing the complex relations among the rhetorical and narrative conventions he analyzes.

The intellectual prominence of artful ethnographers—Geertz, Goffman, and their kin—has heightened attention to elements of style and writing quality. In the process, says Van Maanen, "the sacred power of observation alone" and "the view that ethnography is transparent" have yielded "to an appreciation of the narrative features of the text." Van Maanen uses the folksy term "tales" to highlight the selective representations and stories intrinsic to fieldwork accounts. His own position on whether ethnography is more an objective science or an ambiguously presentational and interpretative art is unambiguous: "This is a phenomenological war whoop declaring that there is no way of seeing, hearing, or representing the world of others that is absolutely, universally valid or correct" (p. 35).

Van Maanen analyzes ethnographic writing not on the basis of calling—anthropology, sociology, or journalism—but on the basis of the narrative conventions he finds employed in the telling of tales, primarily in what he calls realist, confessional, and impressionist tales.

Realist tales are those respectable, dispassionately documented descriptions—and sometimes explanations—of cultural practices narrated in the third-person voice. Contemporary realist tales display carefully edited quotations intended to demonstrate that the views presented are authentically those of the people studied, not the field-worker's. While he

criticizes their ideological claim to scientific objectivity, Van Maanen stresses that ethnographers "in this tradition have created masterpieces that have lived very long lives . . . the durability of some realist work indicates that despite the invisibility, high-science stance, or interpretive omnipotence of the author, the tale is fundamentally sound" (p. 54).

The confessional tale establishes an intimacy with readers by revealing the author's personal feelings and self-absorptions surrounding the culture studied. In doing so, it challenges the view that the social world is composed of "neutral, objective, observable facts." The autobiographical detail argues that the native's point of view can be grasped only when the author comes to see the world in a new way. The confessional tale marks "a fundamental turning point in American social thought" because it recognizes that social facts "are human fabrications, themselves subject to social inquiry as to their origins. . . . Fieldwork is an interpretive act, not an observational or descriptive one" (p. 93).

The impressionist tale is distinguished by its novelistic character. The tellers of these tales use dramatic recall and striking stories of the fieldwork experience as "a representational means of cracking open the culture and the fieldworker's way of knowing it so that both can be jointly examined. Impressionist writing tries to keep both subject and object in constant view" (p. 102).

Van Maanen introduces chunks of his own writing on police organizations to illustrate the three main types of ethnographic representation. His book is brief but complex, moving within chapters from documentary realist to confessional to impressionistic styles; it itself embodies the styles and their conventions and includes a compelling metaconfessional on his own confessional style.

The only qualm I have about the book is its narrative tendency, despite the rhetorical complexity of the three core chapters, to present these forms as discordant. Van Maanen sensitizes the reader to substance presented through style, but his sequential and contrasting treatment of ethnographic genres in the book's larger structure magnifies opposition between forms. For the sake of organization this was probably unavoidable, and at various places he takes pains to counter this division: "Impressionist tales . . . remain very much a subgenre of ethnographic writing. They are typically enclosed within realist, or perhaps more frequently, confessional tales" (p. 106). But the upshot of the argument is that the best contemporary ethnographers use various styles harmoniously. Van Maanen sees that the best way to grasp a layered human reality is with layered technique.

AJS

American Journal of Sociology

Volume 95 No. 2 September 1989

CONTENTS

279 The Strong State, Social Class, and Controlled School Expansion
in France, 1881–1975
MAURICE GARNIER, JERALD HAGE, AND BRUCE FULLER

307 Crime and State Surveillance in Nineteenth-Century France
A. R. GILLIS

342 The Social Meaning of Money: “Special Monies”
VIVIANA A. ZELIZER

378 Occupational Status-Assignment Systems: The Effect of Status on
Self Esteem
WILLIAM A. FAUNCE

401 Similarity of Political Behavior among Large American Corporations
MARK S. MIZRUCHI

Commentary and Debate

425 Setting the Record Straight on Organizational Ecology: Rebuttal
to Young
JOHN FREEMAN AND MICHAEL T. HANNAN

439 Assessing Organizational Ecology as Sociological Theory: Com-
ment on Young
JACK BRITTAINE AND DOUGLAS R. WHOLEY

445 Reply to Freeman and Hannan and Brittain and Wholey
RUTH C. YOUNG

Review Essay

447 Radical Theory and Programmatic Thought
DAVID M. TRUBEK



Book Reviews

453 *Weber, Irrationality, and Social Order* by Alan Sica
CHARLES LEMERT

456 *The Micro-Macro Link* edited by Jeffrey C. Alexander, Bernhard Giesen, Richard Münch, and Neil J. Smelser
TOBY E. HUFF

458 *Fate and Utopia in German Sociology, 1870–1923* by Harry Liebersohn
THOMAS BURGER

460 *Homo Academicus* by Pierre Bourdieu. Translated by Peter Collier
RANDALL COLLINS

463 *Approaches to Social Theory* edited by Siegwart Lindenberg, James S. Coleman, and Stefan Nowak
WILLIAM ROGERS BRUBAKER

466 *Theories of Civil Violence* by James B. Rule
JAMES W. WHITE

468 *The Statistical Analysis of Quasi-Experiments* by Christopher H. Achen
MICHAEL HOUT

470 *The Return of the Native: American Indian Political Resurgence* by Stephen Cornell
JOANE NAGEL

472 *The Hispanic Population of the United States* by Frank D. Bean and Marta Tienda
RICHARD D. ALBA

474 *From Many Strands: Ethnic and Racial Groups in Contemporary America* by Stanley Lieberson and Mary C. Waters
ANTHONY H. RICHMOND

476 *Ethnic Groups in Canada: Adaptations and Transitions* by Edward N. Herberg
STEVEN J. GOLD

478 *The Overthrow of Colonial Slavery, 1776–1848* by Robin Blackburn
SIDNEY W. MINTZ

479 *Landlords and Capitalists: The Dominant Class of Chile* by Maurice Zeitlin and Richard Earl Ratcliff
BETH MINTZ

481 *Social Mobility and Class Structure in Modern Britain*, 2d edition, by John H. Goldthorpe, in collaboration with Catriona Llewellyn and Clive Payne
DAVID B. GRUSKY

483 *State and Revolution in Finland* by Risto Alapuro
JOHN D. STEPHENS

486 *Evangelicalism: The Coming Generation* by James Davison Hunter
TERENCE C. HALLIDAY

488 *Putting Asunder: A History of Divorce in Western Society* by Roderick Phillips
DOUGLAS L. ANDERTON

490 *Uneasy Virtue: The Politics of Prostitution and the American Reform Tradition* by Barbara Meil Hobson
LOUISE A. TILLY

491 *Demographic Behavior in the Past: A Study of Fourteen German Village Populations in the Eighteenth and Nineteenth Centuries* by John E. Knodel
SUSAN COTTS WATKINS

493 *Families and Social Networks* edited by Robert M. Milardo
NORVAL D. GLENN

495 *The Changing American Family and Public Policy* edited by Andrew J. Cherlin
MARK R. RANK

497 *Social Science in Government: Uses and Misuses* by Richard P. Nathan
MARTIN BULMER

499 *The Politics of Social Policy in the United States* edited by Margaret Weir, Ann Shola Orloff, and Theda Skocpol
FRANCIS G. CASTLES

501 *Remaking the Welfare State: Retrenchment and Social Policy in America and Europe* edited by Michael K. Brown
STANLEY DEVINEY

503 *Class Struggle and the New Deal: Industrial Labor, Industrial Capital, and the State* by Rhonda F. Levine
EDWIN AMENTA

505 *American Neighborhoods and Residential Differentiation* by Michael J. White
AVERY M. GUEST

507 *Borderland: Origins of the American Suburb, 1820–1939* by John R. Stilgoe
ADAM BICKFORD

509 *America Becomes Urban: The Development of U.S. Cities and Towns, 1780–1980* by Eric H. Monkkonen
HARVEY MOLOTH

511 *Metropolis: From the Division of Labor to Urban Form* by Allen J. Scott
M. GOTTDIENER

513 *Rainbow's End: Irish-Americans and the Dilemmas of Urban Machine Politics, 1840–1985* by Steven P. Erie
KENNETH FINEGOLD

515 *Searching for Rural Development* by Merilee S. Grindle
DOUGLAS S. MASSEY

516 *Land Reform and Social Change in Iran* by Afsaneh Najmabadi
SAID AMIR ARJOMAND

518 *Women in the World Economy: An INSTRAW Study* by Susan P. Joeckes
CHERYL JOHNSON-ODIM

520 *State and Family in Singapore: Restructuring an Industrial Society* by Janet W. Salaff
DONALD P. WARWICK

522 *Marriage and Inequality in Classless Societies* by Jane Fishburne Collier
NAOMI GERSTEL

524 *Feminization of the Labor Force: Paradoxes and Promises* edited by Jane Jenson, Elisabeth Hagen, and Ceallaigh Reddy
ROSEMARY CROMPTON

526 *Deceptive Distinctions: Sex, Gender, and the Social Order* by Cynthia Fuchs Epstein
LINDA D. MOLM

528 *A Fallen Angel: The Status Insularity of the Female Alcoholic* by Florence V. Ridlon
BARRY GLASSNER

529 *Learning to Heal: The Development of American Medical Education* by Kenneth M. Ludmerer
JACQUELYN LITT

531 *The Diffusion of the Medical Innovation: An Applied Network Analysis* by Mary L. Fennell and Richard B. Warnecke
JOSEPH GALASKIEWICZ

534 *The System of Professions: An Essay on the Division of Expert Labor* by Andrew Abbott
PAUL DiMAGGIO

536 *Legal Secrets: Equality and Efficiency in the Common Law* by Kim Lane Scheppelle
MARK COONEY

538 *Feminism Unmodified: Discourses on Life and Law* by Catharine MacKinnon
PAULINE B. BART

539 *The Noble Dream: The Objectivity Question and the American Historical Profession* by Peter Novick
STEPHEN TURNER

542 *The Rhetoric of the Human Sciences: Language and Argument in Scholarship and Public Affairs* edited by John S. Nelson, Allan Megill, and Donald N. McCloskey
KEITH DOUBT

544 *Ritual Healing in Suburban America* by Meredith B. McGuire, with the assistance of Debra Kantor
Animal Liberators: Research and Morality by Susan Sperling
JAMES M. JASPER

546 *Making History: The American Left and the American Mind* by Richard Flacks
RICHARD A. DELLO BUONO

IN THIS ISSUE

MAURICE GARNIER is professor of sociology at Indiana University. He is working on a project funded by the National Science Foundation that seeks to develop an empirically based theory of educational expansion, using data from Britain, France, Germany, and Italy. Another part of the same project involves the study of the consequences of educational expansion.

JERALD HAGE is professor of sociology in the political economy program and director of the Center for Innovation at the University of Maryland. He is coauthor of two books on the role of the state that are to be published this year. They are *State Responsiveness and State Activism* (with Robert Hanneman and Edward Gargan) and *State Intervention in Health Care* (with J. Rogers Hollingsworth and Robert Hanneman). His article in this issue (written with Maurice Garnier and Bruce Fuller) is the second of a series on the role of the state in education and its consequences for class formation, upward mobility, and economic growth.

BRUCE FULLER recently moved from the World Bank to the U.S. Agency for International Development. His research centers on the historical interaction between the state, economic change, and school expansion. He also occasionally tries to make sense of what is going on in African classrooms.

A. R. GILLIS is professor of sociology at the University of Toronto. His current research focuses on the relationships between urbanization, industrialization, capitalism, and repression/control in several different regions of France, 1865–1913. He is also continuing his research on the relationships between population density, stress, and juvenile delinquency and (with John Hagan and John Simpson) his work on power-control theory.

VIVIANA A. ZELIZER is professor of sociology at Princeton University. Her continuing theoretical interest is in the relationship between economics and sociology. Her two previous books, *Morals and Markets: The Development of Life Insurance in the United States* and *Pricing the Priceless Child: The Changing Social Value of Children*, and her current project on the social meaning of money are efforts to develop a sociological account of economic activity.

WILLIAM A. FAUNCE is professor of sociology at Michigan State University. His research focuses on the relationship between status assignment and self esteem maintenance processes and on continuing industrialization in developed societies.

MARK S. MIZRUCHI is associate professor of sociology at Columbia University. His research includes an examination of corporate political behavior and a longitudinal study of organizational responses to capital dependence. He is the author of *The American Corporate Network, 1904–1974* (Sage, 1982), coeditor (with Michael Schwartz) of *Intercorporate Relations* (Cambridge, 1987), and author of numerous articles.

Information for Contributors

Editorial Procedures: All papers deemed appropriate for the *AJS* are sent out anonymously to readers. The editors rely heavily on the judgments of those readers, although they are not bound by them. To protect anonymity, only the title should appear on the manuscript. Attach a cover page with the title of the manuscript, your name, and your affiliation. All identifying references and footnotes should appear on a separate page. A legible, carefully prepared manuscript will facilitate the work of readers. Manuscripts are accepted for publication subject to nonsubstantive editing with the understanding that the *AJS* has the right of first publication. Submission of a manuscript to the *AJS* is taken to indicate the author's commitment to publish in this *Journal*. No paper known to be under consideration by any other journal will be reviewed by the *AJS*. Upon publication, all rights, including subsidiary rights, are owned by the University of Chicago Press. The author retains the right to use his article without charge in any book of which he is the author or editor after it has appeared in the *Journal*.

Manuscript Acceptance Policy: While it is our policy to require the assignment of copyright on most journal articles and review essays, we do not usually request assignment of copyright for other contributions. Although the copyright to such a contribution may remain with the author, it is understood that, in return for publication, the journal has the nonexclusive right to publish the contribution and the continuing right, without limit, to include the contribution as part of any reprinting of the issue and/or volume of the journal in which the contribution first appeared by any means and in any format, including computer-assisted storage and readout, in which the issue and/or volume may be reproduced by the publisher or by its licensed agencies.

Preparation of Copy

1. Type *all* copy—including indented matter, footnotes, tables, and references—*double-spaced*, allowing generous margins at top, bottom, and sides of page. Please do not break words at ends of lines or justify right-hand margins. Indicate *italics* by underlining only. Number all footnotes, including acknowledgment footnote (which should be “1”).
2. Type each table on a separate page. Refer to each table in numerical order in the text. Prepare tables without vertical lines. Tables should not contain more than 20 two-digit columns or the equivalent.
3. Clarify all mathematical symbols (e.g., Greek letters) with words in the margins of the manuscript.
4. Draw figures on white paper with india ink. Original or glossy print of professionally drawn figure will be required if manuscript is accepted.
5. Include a brief abstract (not more than 100 words) summarizing the findings.
6. *Four copies* of the manuscript and abstract, on white paper, must be submitted. Manuscripts should be addressed to the Editor of the AMERICAN JOURNAL OF SOCIOLOGY, 1130 East 59th Street, The University of Chicago, Chicago, Illinois 60637. *Manuscripts are not returned to authors.* Enclose a self-addressed stamped postcard, containing the manuscript title, to obtain acknowledgment of manuscript receipt. No other acknowledgment will be sent.

7. Each article submitted to the *AJS* must be accompanied by a check or money order for \$15.00, payable to The University of Chicago Press in U.S. currency or its equivalent by money order or bank draft. Papers will not be reviewed until this fee has been paid. The submission fee will be waived for papers solely by student authors. Submissions from students must be accompanied by a faculty member's certification of their student status. Citizens of countries with restrictions on the export of U.S. dollars may request waiver of the submission fee.

Format of References in Text: All references to monographs, articles, and statistical sources are to be identified at the appropriate point in the text by last name of author, year of publication, and pagination where appropriate, all within parentheses. Footnotes are to be used only for substantive observations. Specify subsequent citations of the same source in the same way as the first one; do not use "ibid.," "op. cit.," or "loc. cit."

1. When author's name is in the text: Duncan (1959). When author's name is not in text: (Gouldner 1963).
2. Pagination follows year of publication: (Lipset 1964, pp. 61–65).
3. For more than three authors, use "et al." For institutional authorship, supply minimum identification from the beginning of the complete citation: (U.S. Bureau of Census 1963, p. 117).
4. With more than one reference to an author in the same year, distinguish them by use of letters (*a*, *b*) attached to the year of publication: (1965*a*).
5. Enclose a series of references with a single pair of parentheses, separated by semicolons.

Format of References: List all items, including machine-readable data files (MRDF), alphabetically by author (providing the full list of multiple authors) and, within author(s), by year of publication, in an appendix titled "References." Examples follow:

Davis, James Allan. 1978. *General Social Survey, 1972–1978: Cumulative Data (MRDF)*. NORC ed. Chicago: NORC. Distributed by Roper Public Opinion Research Center, New Haven, Conn.

Davis, K. 1963*a*. "Social Demography." Pp. 124–37 in *The Behavioral Sciences Today*, edited by Bernard Berelson. New York: Basic.

—. 1963*b*. "The Theory of Change and Response in Modern Demographic History." *Population Index* 29:345–66.

Goode, W. J. 1967. "The Protection of the Inept." *American Sociological Review* 32:5–19.

Moore, Wilbert E., and Arnold S. Feldman. 1960. *Labor Commitment and Social Change in Developing Areas*. New York: Social Science Research Council.

Sanford, Nevitt, ed. 1962. *The American College*. New York: Wiley.

U.S. Bureau of the Census. 1970. *Census of Population and Housing 1970, Summary Statistic File 4H: U.S.* (MRDF). DUALabs ed. Washington, D.C.: Bureau of the Census. Distributed by DUALabs, Rosslyn, Va.

Weber, Max. (1921) 1968. "Society's Problems." Pp. 12–16 in *Economy and Society*, edited by Guenther Roth and Claus Wittich. New York: Bedminster.

The Strong State, Social Class, and Controlled School Expansion in France, 1881–1975¹

Maurice Garnier
Indiana University

Jerald Hage
University of Maryland

Bruce Fuller
Agency for International Development

Current theories argue that school expansion reflects demands that stem from changes in the occupational structure, from an ideological commitment to building an integrated nation-state, or from the processes of modernization. However, these theories have not been tested relative to different social classes. Since most of the evidence has accumulated in the United States, where demand is usually met by supply, the possibility that demand might not be met has been ignored. This article relies on the concept of controlled expansion, testing whether the policies and politics of the central French state influenced the expansion of secondary schooling between 1881 and 1975. The state's policies regarding the supply of pupil spaces and the improvement of quality affected actual enrollment growth when the influence of labor structure and institutional values is controlled. Importantly, both economic demand and state supply exert differing levels of influence on the elite school system versus the mass system.

The spectacular increase in the proportion of young people attending school has created a vast empirical and theoretical literature seeking to

¹ The authors wish to thank the National Science Foundation for the original grant that made the collection of the data and the present grant that made the preparation of this paper possible. We also wish to thank the University of Maryland for computer support and for funds to help in the preparation of the French data. The substantial assistance of Chip Melvin is gratefully acknowledged. Pamela Walters read an earlier version of this paper and provided useful comments. Requests for reprints should be sent to Maurice Garnier, Department of Sociology, Ballantine Hall, Indiana University, Bloomington, Indiana 47405.

© 1989 by The University of Chicago. All rights reserved.
0002-9602/90/9502-0001\$01.50

American Journal of Sociology

document the nature of that phenomenon and to explain it (Walters 1984; Meyer et al. 1979; Craig 1981). Most of the empirical research has emphasized either the pull of job opportunities resulting from changes in the labor force, unemployment, and wages or the push of family values about education as reflected in religious preference, ethnic status, or status competition (Meyer et al. 1979; Collins 1979; Walters 1984).

Such an approach assumes that supply will automatically follow the demand. Yet such an assumption has not received empirical attention (except by Prost 1986). The focus on the United States has distracted our attention from the role a state can play in either facilitating or restricting school expansion (Ramirez and Boli 1987; Rubinson 1986). Archer (1982) assumed that the state plays a role in creating a supply only during the initial phase of expansion, later merely responding to demand, but such a view ignores the possibility that the state may intervene constantly by either acting or failing to do so.

The emphasis on the United States has also resulted in another omission: class. As Rubinson (1986) has argued, American education is unusual in many respects, an important one being that it is not nearly as class based as is education in Europe. Thus, it can reasonably be anticipated that the state's educational policies will be affected by class considerations in societies where multiple systems of education exist. It is likely that strong states will exacerbate the class nature of education.

This article has three objectives. First, it seeks to ascertain whether demand models operate similarly in mass as opposed to elite secondary school systems, using France between 1881 and 1975 as a test case. We note that French education was organized in ways that are similar to those of all other European systems until recently. Therefore, such a case is more representative of world patterns than is the United States. Second, it seeks to measure the role of supply (which results from state policies) in school expansion. Third, it ascertains the effect of state policies on the relative importance of different types of demand.

Our general assumption is that, when a strong state controls expansion, it changes the willingness of people to participate in educational systems. Therefore, the particular models that have been proposed by previous researchers will require modification.

France is a useful setting in which to observe the effect of a strong state, at least relative to education. That state has a large bureaucratic apparatus that has resisted pressures (Prost 1968) and limited supply (Delorme and Andre 1983), especially in the elite secondary system. The French educational system is also noteworthy because there existed, and still do, two secondary school systems; one leads to university admission, the other does not. It is possible that each system responds differently to societal forces and that the state adheres to different policies for each of

Schools

them, in other words that supply considerations differ for the two systems.

DETERMINANTS OF EDUCATIONAL DEMAND

Previous educational expansion research has focused on various sources of demand within the context of one educational system available to everyone. After reviewing the major findings, we will speculate about the effect that the existence of elite and mass systems of education may have on demand. We will subsequently examine the effects of state action (educational policies) on these two systems. We will argue that when the state restricts access, then the individuals' calculations concerning the desirability of education become far more complex and involve greater risks than when education is open.

Mass and Elite Systems and the Effect of Job Opportunities

Both the human capital and the credentialist perspectives argue that diplomas are necessary for individuals to enjoy greater job opportunities (Walters 1984; Robinson and Ralph 1984). The two perspectives disagree on the reasons, however. Human capital theorists claim that skills learned in school result in higher productivity (hence, higher wages) (Becker 1964; Denison 1974), while credentialists suggest that employers demand higher and higher qualifications for the same job as a way of screening out an increasing number of qualified applicants (Collins 1979).

These two perspectives generally explain why individuals will seek additional educational credentials, but they do not consider the possibility that members of different social classes may face different opportunities or that the calculations differ when both elite and mass systems of education exist.

Boudon (1974) has suggested that members of different social classes will follow different strategies when deciding to seek additional schooling because each faces a different type of cost/benefit. The existence of two educational systems is one element in the calculus, job opportunities another. An academically talented working-class boy (the issue is even more complex for girls) may believe that his chances of staying the full duration of elite secondary schooling and passing the examination at the end are good. University admission becomes automatic, and job opportunities will be great. Indeed, that boy may well receive powerful encouragement from his teachers and school officials. However, the risk quickly increases as academic performance decreases, for the expenditure of time and hard cash (and forgone earnings) are sizable costs. In addition, the cost of leaving one's class of origin must also be taken into consideration.

Such a boy may therefore decide that he should not seek additional schooling or, alternatively, that the mass system may suit his needs better, for that system will involve fewer costs. The existence of a mass system does not thwart mobility aspirations; it merely restricts their range in return for a more certain outcome. The reverse holds true for the middle-class boy for whom the likelihood of failure must be high before he would willingly give up his chances of remaining in his class of origin. For him, the cost of staying in school is relatively low, the rewards potentially high (all occupations), and the cost of not attending extremely high (the loss of status).

Following Boudon's insight, we surmise that people who pursue elite secondary education should be more sensitive to job opportunities than those who attend a mass school system because all jobs are available to the former, but not to the latter. It is also quite reasonable to expect that different economic sectors will exhibit different appeals to the respective products of the educational systems. We would expect that the growth of the service sector might have greater influence on elite school expansion because the service sector (including government service) usually requires a degree and has historically enjoyed very high prestige in France. In contrast, we might anticipate that the expansion of the industrial sector and the emergence of large-scale enterprises would affect the expansion of a mass system, which produces clerks, technicians, and foremen.

Economic downturns should also affect the expansion of the two educational systems differently. Walters (1984) has shown that wages and full employment pull youths into the labor force, while unemployment can result in the reverse, keeping youths in school until the next expansion occurs (Fuller 1983). This is the "warehousing" hypothesis, called "parking" by Barbagli (1982; see also Rubinson and Ralph 1984). When two educational systems exist, there will be continued incentive to stay in school for those who attend the elite system, but increased disincentive for those who attend the mass system. In addition, given the high representation of the middle class in the elite system, there will be a tendency for that system not to be affected by economic downturns. For that class, schooling is an essential element in the maintenance of status, and that class possesses resources that enable its children to remain in school.

We have modified the traditional argument about the relationship between educational expansion and the organization of work and production to suggest that, when there is both a mass and an elite system of education, the evaluation of the rewards of school differs. When the possibility of getting the degree that controls access to good jobs is remote and uncertain, is it reasonable to stay in school? On the contrary, is it not logical to want to stay in school a long time when the most prestigious degree and jobs loom as distinct possibilities?

Schools

American (but also European) evidence suggests another causal sequence, namely, that educational expansion preceded industrial growth (Robinson 1986; Meyer et al. 1979). France industrialized slowly until World War II, when an economic miracle occurred, as elsewhere in Europe. Such a causal order suggests that school expansion was influenced not by job opportunities, but instead by the perceived value of schooling.

This perceived value of schooling will of course be affected by class membership. Thus, the middle class will value education, and we suggested earlier that such demand was at least as much due to the occupational opportunities of the middle class as to the wish of that class to demarcate its status from that of the other classes. The state may thus face strong demand from the middle class to expand educational opportunities. The working class, however, because the jobs its members can reasonably expect to have do not require academic credentials (apprenticeship was very prevalent in most of Europe), may resist schooling, and the state may be forced to insist on attendance.

Mass and Elite Systems and the Effect of Values

Meyer and his associates (Meyer et al. 1979; Ramirez and Boli 1987; Ramirez and Meyer 1980) have suggested that education can be valued for itself and that adherence to this value in turn will lead to school expansion. Much of their evidence comes from analysis of the United States and demonstrates the effect of Protestant religion, immigration, and urbanization on school expansion. Other evidence from the Third World indicates that schooling is a core value among states seeking to develop (Ramirez and Boli 1987).

The process of modernization can be viewed as involving the diffusion of the myth of education to larger and larger segments of the population (Lerner 1958). Education plays a key role in that diffusion by enabling the middle class to impose its standards as the relevant ones for success in school and, in turn, tying educational success to occupational placement. In order for the working class to function in the society, its members must therefore adhere to the rules of the game (Bourdieu and Passeron 1977). Hence, we would anticipate that this phenomenon is much more important for the working class than for the middle class, which is already educated and whose status depends on that education. Thus, we would expect indicators of modernization to affect working-class expansion, while they would have no effect on the middle class. Even though the working class will attend the mass system that limits access to good jobs, parents can nevertheless observe the link between education and occupational success in the elite system and thus encourage their children to stay

American Journal of Sociology

in school longer. Such a situation would be particularly prevalent among the highly skilled members of the working class who are likely to realize that the on-the-job training they received will no longer be satisfactory in the future.

In a society such as France, where the state makes the linkage between education and employment very tight (by means of legislation and enforcement of private contracts that allocate benefits on the basis of educational credentials), the benefits of education will become even more visible. Hence, even though students of one system may benefit far more than the others, the greater benefits accruing to the more privileged members will nevertheless serve to disseminate the myth of education throughout the society.

Such dissemination is reinforced by urbanization, which facilitates communication and exposure to new ideas and ways of doing things and which offers more educational opportunities than do rural areas. Modernization (i.e., increased urbanization, communication, and transportation) makes the working class aware of the value of education and of its importance for access to jobs, even those of lower status.

Consistent with our argument about status competition's being an essential consideration for the middle class, we would not expect modernization indicators to have much effect on expansion in the elite school system. As far as education is concerned, the middle class is already modern. We would therefore anticipate that family values concerning education (as measured by parental enrollments 25 years earlier) would better explain elite expansion than would indicators of the modernization process.

Thus, two alternative, but complementary, processes make possible the circulation of the value of education in a society. One takes place within the family and is caused by concerns about the maintenance of status (Blau and Duncan 1967). Another process involves the diffusion of modern values about education—whether it is viewed as the road to higher pay or prestige or not—to the working class. This process occurs as a result of the spread of urbanization, communication, and transportation, which heightens the awareness of opportunities. Rising expectations, therefore, can lead to rising aspirations and then to increased enrollments (Lerner 1958).

STATE ACTIONS AND SUPPLY

Before discussing the issue of educational supply, we will outline a number of factors that substantiate our claim that the French state is strong, that is, capable of following its own agenda to a very significant extent. We believe that a description of several features of the French state will

be more convincing than quantitative indicators. In particular, quantitative indicators would fail to give appropriate importance to the ideology of state bureaucrats and to the mechanisms available to them to ensure that certain policies are adhered to (Talbott 1969).

1. *Relative power and legitimacy of bureaucrats.*—Whenever the French parliament passes a law, its methods of implementation must be drafted by senior civil servants and approved by the relevant minister. This gives enormous power to civil servants. Further, it must be noted that these senior civil servants are not perceived as unimaginative bureaucrats but rather as the finest products of an educational system that, historically at least, has defined itself as among the very best in the world. Senior civil servants are selected on the basis of very rigorous admission examinations, and their careers are based on merit. Education and the civil service have always been highly competitive, and, thus, their best products are defined as highly competent men (and now women as well) who share one important characteristic: the interest of the whole country. Indeed, French senior civil servants pride themselves for being above politics (Suleiman 1976). It is simply not possible, under these circumstances, to ignore the opinions and wishes of such people.

2. *Weakness of parliament.*—It should also be noted that, until recently, parliament was highly factionalized and thus could not usually provide the kind of sustained leadership educational policies require. Members of parliament had neither the staff nor the influence to force senior civil servants to follow specific policies, such as opening schools in particular locations.

3. *Administrative centralization.*—France is divided into educational districts (*académies*), each headed by a *recteur* who is appointed by the minister of education. He represents the central state in his area and maintains close communication with ministerial staff in Paris and with the minister himself. This *recteur* is assisted by a staff, and his influence reaches into every school. These *recteurs* are former teachers (who must hold the prestigious state doctorate, the legal requirement to hold a professorship) who are selected, in part, because they share basic educational assumptions. Such a selection of organizational leaders, in addition to their having effective organizational control over all schools in their district, has a number of consequences.

One consequence is that change is likely to be slow, and this would be particularly true for fundamental changes, such as an alteration of educational philosophy. In particular, changing to a nondifferentiated secondary system would be unlikely. Indeed, it took the events of World War II and rapid economic change before French schools decreased their reliance on a dual track system.

But some basic philosophical assumptions remain. For example, the

curricular barriers (whether it be philosophy or mathematics) and an emphasis on quality control remain. An examination of laws and regulations reveals a preoccupation with secondary schools: What training should teachers receive, what disciplines should be included, and how should foreign languages be taught? These concerns involve the elite system far more than the other, for the elite system, started by Napoleon, has always seen its mission as the production of the future elite. When such educational philosophies are shared by most decision makers and when these decision makers are themselves quite able and at the head of an effective civil service system, there will be little incentive to change. This absence of change implies that the state will take some actions (maintaining the status quo) but that it will also fail to act. We consider both outcomes to be policies.

A strong state can affect the calculations of individuals about the worth of education in the following ways: (1) through the establishment of multiple educational systems, (2) by the restriction of access, and (3) by the control of expansion in both systems mandated by the overarching concern with educational quality.

Previous explanations of school expansion assume that the state or other agencies (whether public or private) necessarily respond to changes in demand. Historical evidence, but also recent evidence from the Third World, suggests that, even when they are willing to pay little attention to quality of the education offered, educational providers do not necessarily provide what is demanded. Educational history also suggests that educational providers sometimes anticipate demand (Tyack 1974). In other words, the study of educational expansion must examine supply.

When the state is strong and the educational system differentiated into highly separate school systems, and when there is concern over quality, we would expect the dynamics of growth in the elite and the mass systems to differ. Elite-system growth will be determined by parental concerns over the maintenance of status and the material incentives that derive from educational attainment, for prestigious positions are available only to those who have successfully managed the appropriate educational career.

In the mass system, material incentives cannot be an inducement because these incentives are not available to them. Parental concerns with status are also less compelling. However, in the mass system, growth will eventually take place as the myth of education spreads, assuming, of course, that the state is willing to create room for students.

How can a strong state restrict supply? First, it can by creating multiple educational systems and restricting access to higher education and to jobs to the graduates of those systems. Such a step removes both material

Schools

and psychological incentives to attend the elite system, which, by definition, is small. Second, it can by creating new programs or schools very slowly and limiting access to them. Third, it can by emphasizing quality through difficult courses that act as screening devices and by establishing difficult examinations that many will fail.

Supply can thus be conceptualized as a series of state actions (policies). Indeed, it is possible to argue that inaction is also a policy. We will argue that these policies result in the reproduction of the class system.

The Strong State and the Organization of the School System

The first and most fundamental policy involves the maintenance of a mass and an elite system. A brief history of state policies toward these two systems follows. This summary draws on Prost (1968), Anderson (1975), Harrigan (1975), Maillet (1974), and Ringer (1979).

Napoleon created lycées that were owned and operated by the state and *collèges* that were owned and operated by the town but regulated by the state. At that time, a few primary schools existed. During much of the 19th century, elite schools, which were only for boys, grew only a little, but primary schools grew rapidly until primary education up to age 13 became compulsory in 1881.

The two systems had quite different curricula. The middle-class system had four primary grades, followed by the seven years of secondary education. Latin was required and the exit exam was the *baccalauréat*. It was possible to enter secondary elite schools after five years of primary schooling, but such a move was relatively rare because lycées and *collèges* ran their own primary schools from which they recruited students preferentially.

In the 1880s, working-class secondary education beyond the sixth grade began spreading, but it was attached to the primary schools in classes called *cours complémentaires* or special schools called *écoles primaires supérieures*. But, just as one had to pass an exam to enter a lycée or a *collège*, an exam was also required to enter the *école primaire supérieure* and *cours complémentaires*.

In the 1940s, the state began to integrate the two school systems by first transforming the *école primaire supérieure*, then abolishing the *cours complémentaires* and creating the *collège d'enseignement secondaire* (CES) (grades 6–9), which everybody must attend. Admission to the lycées (grades 10–12, and special classes that prepare students for selective examinations for entry into the *grandes écoles*) is still highly selective.

Upon completing the lycée, a student may take an examination, the *baccalauréat*, which makes it possible to enter the university. Before

World War II, only those who had attended a lycée or *collège* could hope to pass the *baccalauréat*, whose failure rate hovered around 30%.

The Strong State and the Control of Expansion

In both systems, the state was very slow to create new programs or school systems, and the failure to do so retarded growth. The male elite system expanded first when the modern curriculum, which eliminated Latin as a barrier, was created, but that process was not completed until the end of the 19th century. That curriculum was made available to women only in 1882, and, in 1925, the prestigious classical curriculum was finally opened to them, thus creating two equal elite systems, one for men, one for women. But these two elite systems required fees until 1930, and they were located in the larger cities only, restricting access because a substantial proportion of the French labor force was employed in agriculture until very recently.

The state adopted a different policy vis-à-vis the mass system. It required people to remain in school to reach a minimum level. However, for a student in the mass system to remain beyond the compulsory years, both entrance and exit examinations were required. However, these mass schools were free and were located both in cities and in relatively small towns, making them available to a greater proportion of the population than were the elite schools.

We have already mentioned some of the major steps in expansion for the mass system, namely, the creation of the mass secondary schools in the 1880s and 1890s. From this point on, the major expansions included the extension of compulsory schooling from age 13 to 14 and, in 1959, to 16. Naturally, these moves did not pertain to middle-class children, who already stayed in school beyond that age.

In the late 1950s, the state attempted to eliminate cultural barriers with the creation of comprehensive schools, called *colleges d'enseignement général* (CEG), which group all children in grades 6–9. Such a move was designed to facilitate expansion but also to make it easier for children to move on to lycées, which, under the reorganization, included grades 10–12 and the traditional special classes that prepare the best students for the prestigious *grandes écoles*.

The French state's policy toward the two school systems was to constrain growth in the elite system but to be more expansionary in the mass system. However, even in the mass system, the state controlled supply at particular times.

Controlled expansion also prevailed in the technical and vocational schools when they were created in the 1880s and the 1890s. Exams were

Schools

required and fees were charged for the most prestigious schools, the *écoles professionnelles*. As a consequence, working-class enrollments in them were never large. This pattern of creating an opportunity—a new school system—and then carefully controlling with a set of barriers how many could take advantage of it is a typical French one.

Gradually, restrictions on expansion were lifted between 1880 and 1975. The barriers were lifted first in the mass system and only later in the elite system. The consequence is that expansion occurred primarily in the mass system and only secondarily in the elite one.

The Strong State and the Use of Curriculum as Supply

The French state also limited supply by manipulating the curriculum. We have already mentioned the use of Latin in the 19th century. Now, mathematics provides admission into the most prestigious curricula and schools. Strict notions about what should be taught and by whom will necessarily reduce the supply (for a discussion of school knowledge, see Whitty 1985). Children are required to write essays, usually presenting a thesis, antithesis, and synthesis. The mathematics program is very demanding. Thus, by specifying, in great detail, what was legitimate and not legitimate to teach in secondary schools, by quickly becoming the only producer of trained primary school teachers (through the *écoles normales*), and by creating extremely strict certification criteria for secondary teachers, the state reduced the supply and, perhaps, the demand as well.

Unless a working-class child received very powerful encouragement from his teachers, he (and even less she) could not reasonably assume that he would succeed in a curriculum that involved a lockstep approach to all subjects and where a particular weakness (e.g., in mathematics or in Latin) would result in failure to pass the examination (in all subjects, not just in the weak one). Under such circumstances, many young people might simply decide that secondary education was not for them.

Therefore, the setting of extremely strict educational standards that everybody must meet restricts educational opportunities only to those who are academically gifted at the appropriate age (there is no room for late bloomers in such a system).

Indeed, the notion that parents could independently decide what is suitable for their children is not quite legitimate in France. Teachers act as sponsors and gatekeepers (Turner 1960). It should also be noted that such a restrictive policy was also used in vocational and technical education, whereas in the United States these became important channels of upward mobility.

American Journal of Sociology

The Strong State and the Concern about Quality

While it is extremely hazardous to discuss the quality of educational offerings, nevertheless we note that the French state followed very different policies toward the two systems in several key areas. Most important, the student/teacher ratio in the elite system was significantly lower than in the mass system (15 to 1 vs. 30 to 1). Such a practice must have had important implications for retention and success rates, particularly in light of the greater selectivity of the elite system.

In addition, the state constantly tinkered with the elite schools' curriculum and with the training required of teachers in that system. While difficult to measure, such tinkering reflected a preoccupation with the issue of quality of education. We should also note that such a preoccupation stemmed from the assumption that elite schools were designed to educate the future elite (which they did), and hence worthy of lavish care and attention.

The issue of quality has financial consequences. Low student/teacher ratios necessitate large expenditures or, alternatively, restricting the number of students in the expensive system. Such a consequence is particularly noteworthy if it is realized that the preoccupation with quality led the French state to increase teachers' qualifications significantly in the elite system. These teachers, many of whom became prominent intellectuals (Sartre among them), had to receive adequate compensation.

The Reproduction of the Class System

State control over school expansion has enormous class implications. It is possible for the state to control expansion of the elite system, and hence to limit opportunities for upward mobility. Such limitations can be used to strengthen class boundaries not only by reducing opportunities for young people not already members of the middle class but also by legitimating class membership since that membership is dependent upon performing well in a system designed to be demanding (Bourdieu and Passeron 1977).

While academic rigor and a demanding curriculum constituted an important barrier to expansion, another existed, and that one was economic. It must be remembered that France remained a rural country until quite recently and that secondary schools were located in cities. Thus, for many people, the only way of attending a selective secondary elite school was to be a boarder and to pay the attendant fees. Effectively, this meant that only the affluent could afford the expense of sending their sons and daughters. A child had to be truly exceptional in order to receive a scholarship.

The entrance examination into a lycée or *collège* was much harder than

Schools

its equivalent into the *cours complémentaires* or *école primaire supérieure*. No fees were charged in the mass schools and tracks, whereas they were in the elite schools until 1930. As late as 1962, a survey of all French schools sought to ascertain the destination of children at the end of the fifth grade, that is, when the crucial decision to attend or not to attend secondary school was made. Of executives' children, 75% went to the elite school, while only 16% of workers' children did.

The Strong State and the Influence of Politics

What made it possible for these two separate and unequal systems to coexist for such a long time? We argue that a strong state made this inequality possible. There was opposition to the parallel system and agitation for the *école unique* (a single school for all), but educational bureaucrats were able to resist public pressure. French education is centrally controlled through a system of regionally based *académies*, and such an organization makes it possible to resist local pressures. Teachers are civil servants and school administrators' careers are controlled by the bureaucracy and not by local politicians. In addition, routine governance takes the form of decrees and circulars rather than laws passed in parliament (Prost 1968). Given this insulation, we predict that educational policies would not generally respond to political forces.

In addition, the ministry of education is part of a bureaucratic apparatus that can safely be labeled a strong state, as we argued above. Thus, the French state is able to extract financial resources (and to hide a portion of that extraction by relying heavily on consumption taxes), has an administrative apparatus that enables it to carry out its wishes, is able to resist direct political pressures, and is able to attract as state managers the graduates of the most prestigious schools, whose aura of competence further legitimates the state's actions (Suleiman 1976). It should also be remembered that these state managers are the products of French schools and unlikely to question the superiority of the system that produced them, regardless of their own social origins.

DESIGN AND METHODS

This study relies on time series covering a long period. Such a design makes it possible to study the dynamic aspect of the variables and the possible effects of changes in one variable on changes in another. This is particularly important given the long-term unfolding of various policies as well as the slow-moving, almost glacial, pace of social forces such as industrialization and modernization.

We consider the period 1881–1975. Middle-class female schools were

American Journal of Sociology

created in 1882, and the *cours complémentaires* and *écoles primaires supérieures*, secondary schools catering to the working class, were also established then (for historical discussions, see Andersson 1975; Harrigan 1975; Maillet 1974). We close when the two systems merged.

Long time series present problems of data availability. All variables could not be measured during World War II. As a consequence, the years 1939–45 have been eliminated from the analysis. Enrollments continued to grow during the war years in the middle-class schools, however. We inspected the residuals after OLS runs to determine if there were any other outliers (plus or minus two standard deviations). We discovered that there were and added dummy variables to prevent these years from disturbing the analysis. The outliers represent jumps, especially in the working-class system, and the impact of state policies of equality and the resulting reorganization of the school systems in the 1960s, as well as new laws about compulsory education. Despite our best efforts, our estimates of state policies do not capture all of the effect.

Time series produce autocorrelated error, and this general phenomenon is present in our data set. We employed the Cochrane-Orcutt procedure, which, in the process of estimating the parameters of the model, uses an estimate of first-order autocorrelation. The Durbin-Watson statistic was computed to estimate whether rho, the first-order autocorrelation, was zero or not. We used a variety of alternative tests to determine whether these problems affected our findings (Hanushek and Jackson 1977). To determine the sources of autocorrelated errors, we regressed each variable on itself using different time lags. The faster the convergence in that variable, the less it contributes to autocorrelated error. The diagnostics revealed that the major cause of serial correlation error was in the dependent variable, working-class enrollment.

An important problem when using time series involves the estimation of the appropriate lag period. Our best judgment in this case is that educational expansion requires a period of three years before it responds to policies or changes in demand. One reason is that most curricula are organized in three-year cycles, and it seems that educational decisions usually involve not how many years a child will stay in school but in which curriculum that child will participate. Experimentation with different lags indicates that our original hunch—three years—best fitted the data.

Measures

We measured the variables in the following way:

1. *Dependent variable.*—Primary enrollment in the middle-class lycées and collèges was subtracted (the dual track system meant that middle-

Schools

class schools had their own primary schools and working-class schools their own secondary). In effect, we standardized primary and secondary education in the two systems, defining the first five years as primary and the next seven as secondary. The equivalents in the United States would be middle school, that is, grades 6–8, or until age 14, and senior high school. In fact, for a long time, most of the growth in the working-class secondary schools involved growth in the sixth, seventh, and eighth grades. Likewise, there was considerable dropout in the middle-class schools after age 16.

All vocationally oriented schools were included in the category of working-class secondary. The working-class secondary enrollment thus includes the sixth grade of primary school, the enrollments in *cours complémentaires*, *école primaire supérieure*, *école normale*, *école professionnelle*, *école professionnelle du commerce et de l'industrie*. Algerian enrollments were eliminated from the two systems because they were included in the period 1867–1917. In addition, enrollment figures have to be standardized as a percentage of the relevant cohort; otherwise the numbers would merely reflect fertility changes (Craig 1981). We used the ages between six and 14 as the denominator.

2. *Independent variables*.—Our models specify a relationship between economic, political, and institutional variables and educational expansion. The measurement of job opportunities is relatively straightforward. Unemployment and the proportion of the labor force in industry and service are used. Modernization was measured by urbanization, communication, and transportation indices. The sources for these data were various editions of the *Annuaire statistique de la France*, Mitchell (1978), Marczewski (1965), Markovitch (1965–66), Toutain (1963), and Perroux (1955). The references for wages are to be found in Brown and Browne (1968).

Family values about education were estimated by parental enrollment 25 years earlier. Our data set was constructed back to 1800 for both systems and for both sexes. Generally, the various reports of the ministry of education form the bulk of the references.

The policy variables, however, had to be created. Some of the policy changes are reasonably straightforward. For example, in 1942, the Vichy government reclassified the *écoles primaires supérieures* as lycées; that is, they transformed working-class secondary schools into middle-class schools, at least formally. This kind of change can easily be handled by moving the enrollments into the appropriate category. The creation of entirely new tracks represents a more complex problem (e.g., the modern track in secondary schools that made the transition from mass to elite secondary schools possible).

Our procedure estimated the number of persons who would be affected

American Journal of Sociology

by the change (for a description, see Appendix). For example, when female elite schools were created in 1882, we estimated that the maximum potential for enrollment would equal the numbers for males in the same kind of track, namely, the modern curriculum. That potential, in fact, was not realized for 50 years. While such numbers reflect a discrepancy between supply and demand, we realize that the supply could not be filled overnight, and, therefore, we spread the number of persons affected by the policy change over a period of eight years. In short, we gave the state time to build schools and train teachers. Details on these procedures are described in the Appendix.

It is particularly difficult to estimate policies whose intent is egalitarian. We attempted to model two changes, probably the most important, to occur during the entire century: the creation of *CEG* (basically a comprehensive junior high school for all French children) in the 1960s, which attempted to integrate the two-track system; the second change involved the elimination of fees. We created dummy variables for the years 1963–75 (for comprehensives) and for 1931–75 (for elimination of fees).

The measurement of the government's concern for quality is yet another problem. We computed the per capita student expenditure in each track. While there were substantial differences in the amount spent on mass as opposed to elite tracks, a trend toward more equality began in the 1950s. We did not consider technical education, which is usually quite expensive and has no elite equivalent.

Findings

Before we report the effect of different models of demand and supply on enrollments in the mass and the elite schools, it is instructive to examine the dependent variable standardized on the cohort ages six to 14. The explosion that occurred in mass secondary enrollments during the 1960s reflects the requirement of remaining in school until age 16 and, we believe, the favorable influence of comprehensives. The increase of 20% of a cohort within a decade becomes more impressive if contrasted with the earlier trends: it took more than a century for the first 20% increase to occur. It underlines how low the average level of education was before World War II. One of the most noteworthy aspects of the standardization is that elite enrollments are more controlled than mass enrollments.

Making schools free had an effect on elite secondary enrollment, but it was a delayed effect that did not occur until the 1940s. The jump in 1946 was due to two influences: the reclassification of the *écoles primaires supérieures* as lycées and steady expansion during the Vichy government. Further, it is also possible for the state to restrict enrollment, as is clear in

Schools

TABLE 1

SOCIAL CLASS AND EDUCATIONAL EXPANSION: STANDARDIZED COEFFICIENTS

	MASS SECONDARY		ELITE SECONDARY	
	b	t-Ratio	b	t-Ratio
Values:				
Parental enrollment	-.22	-2.73	.19	2.33
Communication.....	.19	1.30	-.37	-2.58
Transportation.....	.21	1.58	.00	.02
Urbanization36	3.38	.01	.01
Job opportunities:				
Wages.....	.27	3.39	.43	5.10
Industrial.....	.15	.96	.27	1.98
Service22	1.87	.29	2.87
Size of firm.....	.05	.73	.35	4.31
Unemployment	-.03	-.42	.06	.93
R ²70		.83
Durbin-Watson		1.03*		2.12†

* Autocorrelated error still exists after a GLS run.

† No autocorrelated error exists after a GLS run.

the 1960s and after. Now we ask if these different patterns of growth are driven by different models of expansion.

Demand, Social Class, and School Expansion

We suggested earlier that two separate models have been presented for predicting the demand for education. The job-opportunities model reflects the expansion in industry and in the service sector, and higher pay or unemployment. The institutional model, however, suggests that people make decisions about education on the basis of values. These values stem from broader societal changes (e.g., urbanization, modernization) that are not necessarily connected to economic changes or come from the family and its concerns about the maintenance of status. We contrast the explanatory powers of these two models in table 1.

As expected, parental enrollment 25 years earlier accurately predicts the expansion of education in the elite schools but not in the mass schools; the betas are .19 and -.22, respectively. In contrast, the processes associated with modernization have a strong effect on mass secondary expansion. Working-class children became aware of the perceived benefits of education through the modernization process.

Job opportunities affect elite expansion more than mass schools. Both wages and size of firm have particularly strong effects on expansion in

American Journal of Sociology

elite schools, while wages encourage expansion in mass schools. More impressive is the combination of positive betas. Except for unemployment, and even that effect is positive, each indication of job opportunities encourages middle-class children to stay in school.

Several reasons may explain this finding. First, the longer time required by an elite school may demand a much greater incentive and more opportunities. Second, job opportunities may actually pull the working-class children (who attend mass schools) out of school, although perhaps not as strongly as they did in the United States (Fuller 1983). Unemployment has no effect.

The economic model of industrialization requires some modification as well. It is expansion of the service sector more than the industrial sector that encourages increased enrollments in both kinds of school systems, but that effect is significant only for the elite system. Industrialization does not require much skilled labor until the late stages, as Landes (1969) has pointed out. Furthermore, it is in the service sector that both government employment and the professions are located—these do demand diplomas—and this is the kind of employment that the middle class wants. We had expected that the mass system would respond more to expansion in the industrial sector and the elite to expansion in the service sector, but that expectation is not borne out by our results. If anything, there is a very slight tendency for the service sector to be the more critical of the two for each social class.

Although not statistically significant, the effect of growth in the service sector on mass-system enrollments is especially interesting. The railroad industry, the post office, and the ministry of education experienced great expansion in the latter part of the 19th century and first part of the 20th, creating opportunities for the working and peasant classes, and all these required education.

The effect of firm size on elite enrollments is another interesting finding. It is the growth of large organizations and bureaucratization (Weber 1968) that influences the expansion of elite schooling, whereas this is only modestly true for the mass schools. As firms grew larger, job opportunities for the middle class were created in the form of managerial and white-collar positions. Bureaucratization thus pulls middle-class children into schools, and their school system expands.

Expansion in mass schools is driven by the modernization process, whereas in the elite it is by job opportunities. It should be remembered that the potential sacrifices are paradoxically greater in the elite schools because the real reward results from passing the *baccalauréat*, which requires seven years and has a passing rate of 66%. In contrast, those who attended mass schools could and did drop out of school more easily.

Between 70% and 80% of the variance is explained in both school

Schools

TABLE 2
STATE POLICIES AND SCHOOL EXPANSION: STANDARDIZED COEFFICIENTS

	MASS SECONDARY		ELITE SECONDARY	
	b	t-Ratio	b	t-Ratio
Policies:				
New programs, compulsory attendance....	.15	2.82	.55	6.95
Student/teacher ratio.....	.71	11.87	.12	1.33
Comprehensives (1963-75).....	.06	1.51	...*	
Free tuition (1931-75).....	...†		.02	.17
Political votes:				
Left party.....	.07	.40	.07	.65
Center party.....	-.32	-2.67	.12	.51
Right party.....	-.26	-1.76	.05	.24
Historical outliers:[#]				
1966.....	-.20	-4.94		
1967.....	-.21	-5.22		
1971.....	.12	2.91		
1972.....	.20	4.61		
<i>R</i> ²90		.33
Durbin-Watson		1.57\$		1.62\$

* Not applicable to middle-class secondary because all CEGs are classified as working-class secondary.

† Not applicable to working-class secondary because it was free except for some technical schools.

Significant outliers in OLS runs.

\$ Indeterminate zone for autocorrelated error.

systems, but there is more autocorrelated error in the model for the mass system. We sought to isolate the variable that produced this autocorrelated error and discovered it was the dependent variable, mass-system enrollments.

State Policies and School Expansion

Table 2 shows that, taken together, state-policy models explain 90% of the variance in mass enrollments, but only 33% in elite. We argue that this is a striking demonstration of the effect of differential state policy. The very small amount of explained variance illustrates the importance of the tight control over growth in the elite system, whereas the opposite was true in the mass system.

This does not make expansionary policies for elite schools irrelevant. The only variable that has an effect is expansion ($b = .36$). Clearly, the creation of female schools and multiple tracks for them stimulated growth in this school system. But this is controlled expansion, with more barriers than opportunities. The extent of the control is illustrated by the limited

effect of free schools. It should be remembered that this policy was implemented at the height of the depression.

State policies strongly affect secondary mass-school expansion. Not only does expansion have a significant beta, but it is quality in particular that strongly influences expansion. For the mass system, quality is the most critical of all the policy variables (b is .56). We suggest that this is because the lower student/teacher ratios create a higher retention rate among children who attend mass schools.

This policy was implemented in the 1950s when the ministry of education began to reduce the student/teacher ratio from 27 to 15, the ratio that prevailed in the elite schools. That reduction took about 15 years to be accomplished. We believe that this policy is one of the reasons for the rapid and explosive growth in the 1960s. Correspondingly, quality does not affect elite schools because they were left unchanged, having had the same ratio since the 1880s.

The policy toward comprehensive schools is a particularly interesting one. It represents the state's attempt to introduce more equality into French education. Table 2 shows no significant effect of this policy on expansion. However, our dummy variable procedure may not be sufficiently sensitive to model the policy. In table 2 we determine whether party votes are associated with expansion. We find that votes for the center reduce mass-schooling expansion. There is little effect on elite enrollments.

Demand, State Policies, and School Expansion

Neither job opportunities nor values exist in a vacuum. State policies will affect the relative importance of these variables. Table 3 reports two models, one of which combines the strongest variables from the previous tables. The explained variance is high for both class systems, about 80% in this second model, which uses relatively few variables.

In the context of state policies, job opportunities have no effect on mass-schooling expansion. In other words, we do not support the parallel finding in the United States, which indicated that the expansion of the economy pulls working-class children out of school (Fuller 1983; Walters 1984). The reverse is true for the middle class. The same parametric structure remains: wages, job opportunities in the service sector, and size of firm all have significant effects on elite expansion.

The inclusion of state policies does not alter the importance of the modernization process in explaining why peasant and working-class children stay in school. The effect of state policies on elite enrollments is to increase each parameter slightly, but to leave the basic structure unchanged: parental enrollment is the major determinant of growth. The

Schools

working class responds to modernization, while, for the middle class, parental enrollment is important.

Conversely, we find that in the context of job opportunities and values, state policies affect the mass system only. Rather than the student/teacher ratio being the dominant policy, we find that all state policies (expansion, including compulsory attendance laws; quality; and comprehensives) affect the expansion of the mass schools. There is little effect of state policies on elite enrollments, but there should not be since the state is attempting to limit that system's expansion.

If we examine the reduced model, which represents the eight most important betas once outliers are controlled, we learn the following: mass secondary enrollments in France during the century being analyzed were driven primarily by the creation of new tracks, school attendance laws, comprehensives, and the modernization process.

Elite expansion is driven primarily by the lure of wages and of job opportunities in large-scale enterprises—the process of bureaucratization. Communication has a strong negative relationship, while parental enrollments have a strong positive relationship. Material incentives push the middle class into school, but these factors are not part of the working-class calculations.

CONCLUSION

We thus find strong support for Boudon's (1974) speculations. In deciding whether or not to attend school, children of the working and middle classes engage in different calculations. For the working class, the risks are high (not earning money, among others) and the rewards not particularly compelling since the type of schooling available to them does not provide access to the most prestigious positions. Selection may also operate since the most talented working-class children may be selected into the elite system. Such processes reinforce the perception that one system is for the talented, the other for the less able.

Therefore, demand models operate differently in mass and elite educational systems. The source of institutional values about education is quite different for the two systems. For the elite, these values come from the family and not from the modernization process. The material incentives are real for the elite, but not for the mass system. It is only the elite educational system that confers the *baccalauréat* and offers access to the most prestigious and highly paid jobs. Both classes are responding to the presence or absence of incentives, and these, obviously, are class related.

Another objective was to determine the role of state policies in explaining school expansion. State policies were designed to preserve the elite nature of the system, and growth, when it occurred, was highly

TABLE 3
SOCIAL CLASS, STATE POLICIES, AND SCHOOL EXPANSION: STANDARDIZED COEFFICIENTS

	MASS SECONDARY				ELITE SECONDARY			
	Full Model		Selected Variables*		Full Model		Selected Variables*	
	b	t-ratio	b	t-ratio	b	t-ratio	b	t-ratio
Parental enrollment.....	-.07	-4.08	...†	...†	.29	3.17	.20	2.43
Communication.....	.14	2.07	.03	.28	-.40	-2.70	-.22	-1.76
Transportation.....	.40	4.51	.53	4.41	.14	.99	-.03	-.23
Urbanization.....	.20	2.99	.34	3.49	-.13	-.91	...†	...†
Wages.....	-.07	-1.26	...†	...†	.36	3.85	.41	4.68
Industrial.....	-.08	-1.72	...†	...†	.15	1.22	.10	.80
Service.....	.00	...†	...†	...†	.17	2.64	.15	1.45
Size of firm.....	-.11	-2.20	-1.06	-1.05	.26	2.64	.18	1.81
Unemployment.....	-.07	-2.54	...†	...†	.05	.75	...†	...†
New program, compulsory.....	.27	6.66	.34	5.46	.14	1.09	.27	2.53
Student/teacher ratio.....	.13	1.55	.03	.40	-.05	-.73	...†	...†

Comprehensives.....	.10	4.80	.15	3.13	.03	...†
Free tuition.....	...§				.63	
Left party vote.....	.05	.82	...†		.36	
Center party vote.....	-.13	-.1.53	-.04	-.14	-.84	.†
Right party vote.....	-.08	-.1.65	...†	-.09	-.84	...†
Historical outliers:						
1966.....	-.07	-.4.86	-.22	-.4.42		
1967.....	-.11	-.7.47	-.41	-.7.75		
1971.....	.04	2.76	.12	2.51		
R ²99		.84		.91	
Durbin-Watson.....	1.65#		1.31**		1.97††	
						2.01††

* Eight variables having the largest effect plus significant outliers.

† Variables dropped because they were low in significance.

‡ Not applicable to middle-class secondary because all CEGs are classified as working-class secondary.

§ Not applicable to working-class secondary because it was free except for some technical schools.

|| Significant outliers in OLS runs.

Indeterminate zone for autocorrelated error still exists after a GLS run.

** Autocorrelated error still exists after a GLS run.

†† No autocorrelated error exists after a GLS run.

American Journal of Sociology

controlled: admission tests, rigorous curriculum, exit examination, insistence on normal progress, while ostensibly designed to control quality, nevertheless acted as barriers. In contrast, in the mass system, the creation of new tracks and especially the reduction of the student/teacher ratio facilitated growth.

Yet another objective was to examine the combination of demand and supply. The combination reinforces the relative differences in the determinants of school expansion, with mass-system enrollments growing through the diffusion of myths about education (facilitated by urbanization and increased communication and transportation, in other words, the modernization process). As a consequence of state policies, middle-class enrollments grew because of the concern about status and in response to material incentives.

We have begun the task of constructing a contingency theory of school expansion by suggesting that future educational research retain the distinctions of mass versus elite systems and the strength of the state and, more critically, its educational policies. The most fundamental educational policy followed by the French state has involved the maintenance of two separate systems. Elite secondary enrollments have been very carefully controlled, and even mass secondary enrollments were not allowed to expand rapidly until the 1960s.

We find it useful to characterize educational systems as demand driven (e.g., the United States) or supply driven (e.g., France). As Robinson (1986) suggested, the more typical pattern is the supply-driven system, where strong states exercise tight control.

Another major conclusion is that state policies or actions do make a difference. Clearly, strong states that restrict supply reinforce the class structure. Robinson observed that a major difference between the United States and Europe involved the strong association, in Europe, between social class and education. He did not, however, explain this phenomenon. We suggest that such a link is created by a strong state that differentiates between elite and mass systems of education.

It should not be assumed that these strong states are uninterested in equality. On the contrary. However, the existence of two separate systems makes it extremely difficult for egalitarian policies to be effective as long as these two systems remain. Some mobility does take place, since elite systems are not entirely closed to the working class, but dual systems do not promote equality. It is therefore no accident that, in 1959, the two systems were merged up to grade 9. Recent evidence suggests that, in fact, that new policy may have had the intended effect (Smith and Garnier 1986).

The shift in state policies over the past century implies a very interesting relationship between state policy and the particular demand mod-

Schools

els. Because the middle class is highly motivated to attend school for status and material reasons, the state attempts to control the number of students and to maintain quality. In contrast, in the working class, given their lack of response to these motivators, the state forces them to go to school and attempts to eliminate barriers. Future research will have to explore whether, in fact, there was a conscious link between the differences in policies and differential concerns about the rates of expansion.

Future researchers on national school systems where the state is strong need to model state policies so that we understand how the state facilitates or hinders the process of growth and makes educational opportunities more or less available. We conclude that state policies are far more important than the existing literature has acknowledged. These policies are important throughout the life of educational systems, not just at their onset. The reasons for this are clear. The state continuously makes policies on the creation of new schools, new systems, and new curricula. These are not just decisions on primary education but apply to all levels and even more so to secondary and higher education. How do these findings compare with the American? Can we integrate them into a single and coherent theory about school expansion?

Our findings do not contradict those pertaining to the United States. We have tried to explain some of the divergence. The most important is the role of the state. State policies are probably more important in nations such as France where the central government is and has been a powerful institution (see Tilly 1975). In contrast, the United States was characterized by a weak central government for much of the country's history and also by weak state governments vis-à-vis local school districts. Thus, in such a setting, demand models are much more likely to be persuasive, for no independent forces control supply—no state bureaucrats concerned about quality, no independent agenda, nor anything else that might thwart or increase demand. Our findings suggest that educational policies at the state and local levels should be investigated in the United States, where, unlike in France, the political variables might have a strong effect on enrollment. Just as they do not have a strong effect in France because the state is strong, they will in the United States because local jurisdictions are weak relative to organized middle-class interests.

APPENDIX

Construction of State Policy Variables for Each School System

For the elite system, the first major change in education was the creation of the modern curriculum. Two grade levels were created in 1829. We estimated that this change could have doubled middle-class enrollments. Therefore, we took 2/7 of male elite enrollment in 1829. The expansions

American Journal of Sociology

occurred in 1847, 1866, and 1891, when it became a full-fledged, seven-year program. Each time, one more grade level was added. In 1882, female elite schools were created. Since the middle-class females were allowed to take only the modern curriculum, we used male enrollment in that track. In 1924, the classical track for females was created. We doubled the current enrollment in female lycées and *collèges*. Finally, in 1942, the *écoles primaires supérieures* were moved from the mass to the elite system. We took their enrollments at that time and subtracted these numbers from the mass enrollments.

An important issue involves the ability of the state to build schools quickly and to staff them. In France, implementation of policy decisions can be remarkably quick. In 1883, one year after the legislation, 23 lycées and *collèges* for women had been created, and their enrollment was 2,937 (*Annuaire statistique* 1913, p. 24). By 1893, or 11 years later, 55 schools existed, with an enrollment of 9,249. We conclude that using eight years for implementation is more than adequate, particularly since the growth in the first years is exponential.

For the mass system, the first major expansion involved the creation of the *écoles normales* to train teachers. We estimated these numbers by taking the total of *communes* (33,000) and dividing it by an average professional life of 20 years. This resulted in 2,000 teachers being needed every year. In 1881, the *écoles primaires supérieures* and *cours complémentaires* were added. Since, in order to enter these schools, students needed a *certificat des études primaires*, we used the number of holders of this degree as an estimate for enrollments. Again, we checked the number of schools created and their enrollments. In 1881, 189 public *écoles primaires supérieures* (EPS) were created, and their initial enrollments were 17,300 students (males and females). By 1891, the number of schools had increased to 281. In this same period, the number of private schools decreased from 54 to 21. Enrollment in the public sector increased to 27,900 students. A parallel change occurred in the *cours complémentaires* (CC), with 291 schools offering these courses in 1881 and 482 10 years later. The initial enrollment went from 7,800 to 14,000 10 years later. In 1892, and a few years before that, a number of technical schools were created, primarily *écoles pratiques du commerce* (EPCT) and *écoles professionnelles*. In this case, the local governments took longer to establish these schools, but within five years, 23 schools existed (with an enrollment of 3,314). Ten years after the legislation, 40 schools had been established, and their enrollment had reached 7,160. Since the *certificat des études primaires* was required, we estimated the potential enrollment as one-half of the holders of this degree on the assumption that many would choose to continue to enter *écoles primaires supérieures* and the *cours complémentaires*. Actually, in the 10 years between 1892 and 1902, EPS did lose

Schools

some of its male enrollments to *EPCI*, reaching 23,425 in 1896 and dropping slightly to 22,870 in 1902. The total enrollment masks this trend because female enrollments grew at a faster rate in this period. The enrollments in *cours complémentaires* show no reduction, moving from 14,000 in 1891 to 21,500 in 1901.

In 1936, the age of compulsory schooling was extended one year, from 13 to 14. We estimated the numbers affected to be 10% of the cohort, earlier evidence suggesting that such policies have little effect on enrollment (Prost 1968).

In 1942, the mass schools lost one track, which became technical, but, in 1948, the addition of *centres d'apprentissage* created a new set of working-class technical tracks. We estimated potential enrollment in these schools to represent one-half of the holders of the *certificate des études primaires*. In other words, as the working class lost one avenue of upward mobility in the *EPS*, it gained another in the *centres d'apprentissage*.

In 1959, compulsory schooling was extended another two years, so we estimated the numbers affected to be 20% of the cohort.

REFERENCES

Anderson, Robert D. 1975. *Education in France*. Oxford: Clarendon.

Annuaire Statistique de la France. Various years. Paris: Documentation française.

Archer, Margaret S. 1982. *The Sociology of Educational Expansion: Take-off, Growth, and Inflation in Educational Systems*. Beverly Hills, Calif.: Sage.

Barbagli, Marzio. 1982. *Educating for Unemployment: Politics, Labor Markets, and the School System—Italy, 1859–1973*, translated by Robert H. Ross. New York: Columbia University Press.

Becker, Gary. 1964. *Education and Jobs*. New York: Praeger.

Blau, Peter, and Otis D. Duncan. 1967. *The American Occupational Structure*. New York: Wiley.

Boudon, Raymond. 1974. *Education, Opportunity and Social Inequality*. New York: Wiley.

Bourdieu, Pierre, and Jean-Claude Passeron. 1977. *Reproduction*. Beverly Hills, Calif.: Sage.

Brown, E. H., and Margaret Browne. 1968. *A Century of Pay*. London: Macmillan.

Collins, Randall. 1979. *The Credential Society: An Historical Sociology of Education and Stratification*. New York: Academic.

Craig, John. 1981. "The Expansion of Education." Pp. 151–213 in *Review of Educational Research*, vol. 9. Edited by David Berliner. Itasca, Ill.: American Educational Association.

Delorme, Robert, and Christine André. 1983. *L'Etat et l'économie: un essai d'explication de l'évolution des dépenses publiques en France, 1870–1980*. Paris: Editions du Seuil.

Denison, Edward. 1974. *Accounting for United States Economic Growth, 1929–1969*. Washington, D.C.: Brookings Institution.

Fuller, Bruce. 1983. "Youth Job Structure and School Enrollment, 1890–1920." *Sociology of Education* 56:145–56.

American Journal of Sociology

Hanushek, Eric A., and John E. Jackson. 1977. *Statistical Methods for Social Scientists*. New York: Academic.

Harrigan, Patrick. 1975. "Secondary Education and the Professions in France during the Second Empire." *Comparative Studies in Society and History* 17:349–71.

Landes, David S. 1969. *The Unbound Prometheus: Technological Change and Industrial Development in Western Europe from 1750 to the Present*. Cambridge: Cambridge University Press.

Lerner, Daniel. 1958. *The Passing of Traditional Society*. New York: Free Press.

Maillet, Jean. 1974. "L'Evolution des effectifs de l'enseignement secondaire de 1809 à 1961." Pp. 115–74 in *La Scolarisation en France depuis un siècle*, edited by Pierre Chevallier. Paris: Mouton.

Marczewski, J. 1965. *Le Produit physique de l'économie française de 1789 à 1913 (comparaison avec la Grande Bretagne)*. Cahiers de l'ISEA, ser AF, no. 4 (July).

Markovitch, T. J. 1965–66. *L'Industrie française de 1789 à 1864*. Cahiers de l'ISEA, ser. AF, no. 4 (July), no. 5 (May), no. 6 (June), and no. 7 (November).

Meyer, John, David Tyack, Joanne Nagel, and Audri Gordon. 1979. "Public Education as Nation-Building in America, 1870–1930." *American Journal of Sociology* 85:591–613.

Mitchell, Brian R. 1978. *European Historical Statistics 1750–1975*, 2d rev. ed. New York: Facts on File.

Perroux, François. 1955. *Prise de vue sur la croissance de l'économie française 1780–1950. Income and Wealth*, ser. 5.

Prost, Antoine. 1968. *Histoire de l'enseignement en France 1800–1967*. Paris: Armand Colin.

—. 1986. *L'Education s'est-elle démocratisée?* Paris: Presses Universitaires de France.

Ramirez, F., and John Meyer. 1980. "Comparative Education: The Social Construction of the Modern World System." *Annual Review of Education* 6:369–99.

Ramirez, F., and John Boli. 1987. "The Political Construction of Mass Schooling: European Origins and Worldwide Institutionalization." *Sociology of Education* 60:2–17.

Ringer, Fritz. 1979. *Education and Society in Modern Europe*. Bloomington, Ind.: Indiana University Press.

Rubinson, Richard. 1986. "Class Formation, Politics, and Institutions: Schooling in the United States." *American Journal of Sociology* 92:519–41.

Rubinson, Richard, and John Ralph. 1984. "Technical Change and the Expansion of Schooling in the United States, 1890–1970." *Sociology of Education* 57:134–52.

Smith, Herbert, and Maurice Garnier. 1986. "An Analysis of the Association between Background and Educational Attainment in France." *Sociological Methods and Research* 14:317–44.

Suleiman, Ezra. 1976. *Les hauts Fonctionnaires et al Politique*. Paris: Editions du Seuil.

Talbott, John E. 1969. *The Politics of Educational Reform in France, 1918–1940*. Princeton: Princeton University Press.

Tilly, Charles. 1975. *The Formation of National States in Western Europe*. Princeton: Princeton University Press.

Toutain, Jean-Claude. 1963. *La Population de la France de 1709 à 1959*. Cahiers de l'ISEA, suppl. N, no. 133 (June).

Turner, Ralph H. 1960. "Sponsored and Contest Mobility and the School System." *American Sociological Review* 25:855–67.

Tyack, David. 1974. *The One Best System*. Cambridge: Harvard University Press.

Walters, Pamela. 1984. "Occupational and Labor Market Effects on Secondary and Post-secondary Expansion in the United States 1922–1979." *American Sociological Review* 49:659–71.

Weber, Max. 1968. *Economy and Society*. New York: Bedminster.

Whitty, Geoff. 1985. *Sociology and School Knowledge*. London: Methuen.

Crime and State Surveillance in Nineteenth-Century France¹

A. R. Gillis

University of Toronto

Historians and other social scientists argue that cultural change and increasing civility generated a five-century decline in rates of violent crime. This study presents a structural perspective that indicates that, as emerging nation-states consolidated power, they also extended surveillance, and this deterred crime. The proposition is tested in France, between 1865 and 1913, where rates of major crimes declined and minor offenses increased as state surveillance expanded in the form of two national police forces. A time-series analysis suggests that, although the growth of policing initially increased charges for all types of crime, for major crimes the long-run effect was deterrence and declining rates. Subsequent deterrent effects failed to compensate for the initial inflationary effect on minor offenses, so rates increased. The effect of policing on major property crimes holds when the possibility of reclassification of such crimes as minor offenses is controlled, but the deflationary effect of policing on major crimes of violence disappears when urbanization is introduced as a control. The other relationships persist, further supporting the idea that policing contributed to the decline in serious property crime. A reversal of the equations shows that crime rates had little or no effect on the growth of national policing. This, and historical evidence, suggests that state surveillance expanded less from a specific intent to control crime than from a broader interest in repressing "dangerous classes," new repertoires of social protest, and political challenge to the state.

¹ An earlier version of this paper was presented at the annual meetings of the American Society of Criminology, November 1987, Montréal. Je voudrais remercier pour leur assistance M. Martin Galipot et le Ministère de la Justice, Paris. Thanks also to the Social Science and Humanities Research Council of Canada and the Program in Legal Theory and Public Policy, Faculty of Law; University of Toronto, for supporting this research. Raymond Breton, John Hagan, Kevin McQuillan, and the anonymous reviewers for *AJS* gave advice or criticism of earlier drafts, as did John Beattie, Helen Boritch, Bill McCarthy, Jeff Reitz, Ian Robertson, and Austin Turk. Most of all, I thank Charles Tilly, who gave so freely of his extensive knowledge and his limited time. Requests for reprints should be sent to A. R. Gillis, Department of Sociology, University of Toronto, 563 Spadina Ave., Toronto, Ontario, Canada M5S 1A1.

© 1989 by The University of Chicago. All rights reserved.
0002-9602/90/9502-0002\$01.50

American Journal of Sociology

Recent increases in rates of common crime are relatively small in comparison with an overall decline in rates of major crimes of violence during the past half millennium (Gurr 1979, 1981; Gurr et al. 1977; Stone 1983; Lane 1974). In view of the magnitude and duration of the changes, the salient sociohistorical question is not why these rates of serious crime seem to have increased over the past few decades but why they decreased so dramatically over the past several centuries. Historians and other social scientists typically explain the great decline in crime as part of a broader cultural change that increased the level of civility in many European populations.

This article presents and tests a structural explanation involving the growth of the nation-state and the expansion of surveillance. Specifically, the study focuses on the growth of national policing in France between 1865 and 1913 and its effect on rates of crime. I examine both contemporaneous and long-term effects of policing on crime, and control for the effect of urbanization, as well as the possibility of change through reclassification in the justice system. I also examine the importance of crime rates as determinants of the expansion of state policing.

CULTURES OF VIOLENCE AND NORMS OF CIVILITY

The long decline in violent crime may reflect important cultural changes that began in Europe with the Renaissance. The medieval period witnessed frequent displays of spontaneous violence, widespread mayhem, and high rates of serious crime (Gurr 1979; Stone 1983), particularly in rural areas (Given 1977). Although punishments were severe by today's standards, authority was haphazardly organized, arbitrarily administered, and thinly distributed within and between the many localities that now constitute modern Europe. The severity of sanctions, including displays of dramatic cruelty, public spectacles such as the pillory and stocks, and permanent stigmatization such as branding and amputation may have represented attempts to offset weak links of communication and irregular enforcement of laws with exemplary punishment. Retribution that was harsh by today's standards may also have been deemed appropriate by people for whom violence was a common occurrence (see Given 1977; Foucault 1973; Stone 1977).

During the Renaissance, nobles who had typically maintained standing armies gave up the practice of martial arts and moved from isolated castles and rural estates to the courts, with their emphasis on emotional control, social graces, political intrigue, and intellectual rather than physical contests (Elias [1939] 1978). Self-control became a valued asset. This

Crime

culture of the court (from which "courtesy" derives) diffused downward from the nobility, through the bourgeoisie and beyond. The "gentle man" was born. As the culture of violence eroded, the European population became more civilized, the individual became more highly valued, and rates of crime began a long decline (see Elias 1978, [1939] 1982, [1969] 1983; also Gurr 1976). In the 19th and early 20th centuries, urbanization imposed further and more widespread pressures for greater self-control and nonviolent ways to settle disputes (see Simmel 1970, on "rational restraints"), which would have entrenched and disseminated the norm of nonviolence widely among urbanizing European populations, particularly in cities (Lane 1980). (See, esp., Miller 1958; Wolfgang and Ferracuti 1967; Banfield 1968; Erlanger 1976 for recent treatments of the "subculture-of-violence" thesis.)

THE GROWTH OF STATES AND THE CENTRALIZATION OF COERCIVE POWER

The civilization of Europe certainly seems to have involved an increase in public preference for nonviolent settlement of disputes. But whether these changes in values caused or accompanied the pacification of European populations is unclear. It is noteworthy that Elias (1978, 1982) frames his analysis of the emergence of civilization in structural change and control theory, beginning with the repression of the nobility by monarchs. The centralization of authority and the increasing coercive power of states, particularly within their own borders, may have produced changes in values and norms and affected rates of crime as well. In England, for example, Henry VII started the largely successful campaign by the Tudors to disarm the nobility and suppress private violence (see Stone 1967), and by 1850, previously accepted forms of violent settlement of disputes, such as the duel, had also died out.

With the decay of feudal estates and local autonomy, the size of political units increased, as did the power of rulers to resist external challenges (see Tilly 1987). The invention and use of artillery, and the development of roads on which to transport the cannons of the state, probably facilitated this, while making most stone castles obsolete as strongholds against the state's new military might (see Tilly 1985). This could have been why diplomacy at court replaced confrontation in the field as a more effective avenue to power, prestige, and property for the nobility. As states grew in size and strength, power became more centralized as local authority structures were subsumed by "more encompassing auspices," and "the total amount of sanctioning to which individuals are subject

American Journal of Sociology

grows as more encapsulating levels of authority undertake sanctioning" (Bayley 1985, p. 41; also Black 1976). The growth of states may not have replaced local organization and control, then, so much as it expanded and intensified the integration and regulation of individuals by broader collectivities. (An increase in the national tax rate, e.g., rarely brings reductions in provincial or municipal rates [see also Foucault 1975].) As nation-states emerged, they consolidated their power and extended their capacity to sanction while reducing the capacity of the opposition to resist. This is "the very essence of government," and its "defining activity" is "the maintenance of order" (Bayley 1985, p. 45).

Giddens (1985) concurs. Generally following Max Weber, he views the nation-state as a "power container," defining it as "a set of institutional forms of governance maintaining an administrative monopoly over a territory with demarcated boundaries (borders), its rule being sanctioned by law and direct control of the means of internal and external violence" (1985, p. 120). He adds that "in nation-states surveillance reaches an intensity quite unmatched in previous types of societal order, made possible through the generation and control of information, and developments in communication and transportation, plus forms of supervisory control of 'deviance'" (p. 312). Giddens recognizes the importance of the state in the civilizing of Europe when he states that "the development of the absolutist state was undoubtedly associated with major advances in internal pacification" (p. 189; see also Bailey 1975; Gillis 1987; Tilly 1987).

The expansion of state surveillance was also tied to urbanization. According to Giddens (1985, p. 190), "Modern policing with its characteristic mixture of informational and supervisory aspects of surveillance was both made possible and seen to be necessary by the wholesale transferral of populations from rural to urban environments." (He sees "heightened surveillance" as one of four "institutional clusterings associated with modernity"—the others are centralized control, industrialization, and capitalistic enterprise.) In fact, Giddens (1985, p. 15) goes so far as to argue that "only in cities could regular surveillance be maintained by the central agencies of the state" (see also Stinchcombe 1963; Foucault 1975). On the other side, Hay (1975) argues that, until the 18th century, surveillance in England was embedded in the social relations between the landed gentry and the peasantry. So the growth of formal policing in the 19th century may represent less a change in the amount than in the form of state surveillance, from a local, more encompassing mode to a more formal and specific institutional one. If this is the case, policing could represent a change in the form of surveillance by emerging states in response either to the decline of a rural, agrarian social order or to the urban industrial conditions that followed it.

REACTION THEORY: DEFINITIONS OF CRIME AND PATTERNS OF ENFORCEMENT

Historical analyses are typically confined to official records or published accounts of incidents, arrests, charges, and the like. Although these accounts may give a generally accurate portrait of visible crime, their validity as a measure of hidden crime and actual levels of criminality is uncertain. The five-century decline in rates of serious crime, then, is "real" only inasmuch as recorded sources of information reflect it, and a variety of factors besides a decline in serious criminal behavior could have deflated the official rates of major crimes.

The emergence of bureaucratic urban states and modern legal systems resulted in much more than intensified law enforcement. As the legal apparatus became more complex, patterns of enforcement and punishment, and the law itself, changed. These alterations resulted from wider cultural, social, and economic changes (see, e.g., Chambliss 1964), but they also had an effect on both the nature and volume of criminal statistics, independent of actual criminal activity. As European states expanded over the past five centuries and people became more civil, both the penalties for infractions and the definition of illegality became more specific. If punishment is used as the basis for classifying crime as serious, the frequency of serious crime may have declined more because of reclassification than pacification (although this seems unlikely in view of post-Enlightenment transformations in penal strategies). Data on convictions or charges, then, may reflect not only a decline in rates of serious criminal behavior but also a growing inclination to redefine specific criminal behaviors as less serious. Changes in legal classification can be found in official records, but an increasing inclination to charge, convict, and punish offenders for less serious crimes may not be easy to identify, appearing only as a decline in the rate of major crimes and a corresponding increase in the rate of minor offenses. Juries may also have been more inclined to convict offenders for minor than major offenses. (See, e.g., Beattie 1986; Wright 1983, p. 96; also Foucault 1975; Ignatieff 1978; and O'Brien 1982 on changing patterns of punishment.)

Changes in judicial procedure and police discretion may also have had an independent effect on the nature and volume of official crime statistics. For minor offenses, the growing concentration of people in cities may have combined with escalating surveillance to increase rates of arrests and charges, independent of any change in rates of behavior that would be adjudicated criminal if detected and processed (see Cicourel 1968; Foucault 1975; Perrot 1975; Stinchcombe 1963; Monkkonen 1975, 1981). Also, as population grew, especially in urban areas, the increasing number of offenders may have resulted in a growing tendency to reduce

charges in an effort to clear overburdened courts, particularly those hearing serious offenses, which consumed more time (see Alschuler 1979; Ferdinand 1967; Haller 1970; Heumann 1975). If this occurred, declines in official rates of serious crimes would have been purchased with increases in official rates of minor offenses.

In summary, the five-century decline in serious crime may have resulted from several factors. Broad cultural change associated with the pacification of the nobility and the concentration of population in cities may have increased general civility and a public preference for nonviolent settlement of disputes. Alternatively, the emergence and growth of states and their monopolization of coercive power may have led to the imposition of law and order on European populations. A third possibility is that increased policing may have inflated the volume of offenders, overburdening courts and increasing pressure on officials to reduce charges to speed up processing. Any or all of these explanations could account for the decline in rates of serious crime in Europe over the centuries.

These three perspectives imply different consequences for crime rates from the expansion of policing. If policing is irrelevant and increasing civility is the principal cause of the decline, there should be little or no correlation between policing and crime rates unless the growth of policing is also a consequence of increasing civility. But if policing deters crime, the correlation should be negative. However, if reaction arguments hold, the most immediate effect of increased policing should be increased arrests, charges, convictions, and official rates of crime because of a greater capacity to detect crime. If this is the case, the correlation between policing and crime rates will be positive. These three possibilities seem on the surface to be mutually exclusive: correlations are positive, negative, or nonexistent. All three outcomes could coexist, however, with different types of crime, and in different stages of the causal sequence at different points in time.

CONTEMPORANEOUS AND LONG-TERM EFFECTS AND TYPES OF CRIME

Most social researchers give little attention to time, if indeed they consider it at all (see Abbott 1983; Garofalo 1981; Lieberson 1985). Yet the causes and effects described by their theories do not by definition occur simultaneously (Nettler 1970). Instead, a change in an independent variable (cause) is followed sometime later by a change in the dependent variable (effect). For example, the immediate result of increased policing is most likely to be an increase, not a decrease, in arrests (see, e.g., Gatrell 1980). If increased policing produces more arrests but also deters crime, these countervailing forces will confound correlations in the same time

period. However, it is reasonable to expect any deterrence to follow both the immediate appearance of the police and their initial inflationary effect on arrests. So the initial effect of policing on crime may be positive and subsequent ones negative. If this eventual deterrence outweighs the initial inflationary effect, crime rates will decline with increased policing.

Policing may also have different effects on different types of crime. Violence is typically more spontaneous and passionate than is property crime, which makes the latter a better candidate for deterrence. Further, since minor offenses typically leave more room for police discretion (Nettler 1984) and are by definition less severely sanctioned than are major crimes, minor offenses may be more susceptible to initial inflationary effects and less susceptible to eventual deterrence from increased policing.

In view of this, the following analysis examines the relationship between policing and crime in the same period of time, when an inflationary effect is most likely to prevail (supporting reaction arguments), as well as over an extended period of time, when deflationary effects are more likely to be exhibited (supporting deterrence theory). Since major property offenses are the most susceptible to deterrence and minor offenses, particularly those involving violence, are the most likely to be inflated by changes in policing, the analysis also distinguishes between crimes of violence and property offenses, in both their major and minor forms.

NINETEENTH-CENTURY FRANCE

This article examines these arguments in a particular historical context: France during the second half of the 19th and first part of the 20th centuries. Because of the focus of the research, France during this period is a particularly appropriate case to analyze. An obsession with decadence and crime "endured at all levels of French society throughout the 19th century" (Beirne 1987, p. 1145), and Durkheim provoked considerable controversy by declaring at least some social pathology inevitable and locating its origins in society and its moral boundaries (see Wright 1983). At the time, prevailing beliefs about crime focused on the emergence and growth of "dangerous classes" as the cause of both criminal and political upheaval (Tombs 1980). It is not surprising, then, that the national police forces also emerged and grew rapidly during this period (Stead 1984). A simultaneous increase in both policing and crime does not suggest that policing had much to do with a decline in crime. However, this is a reason for selecting, rather than avoiding, France as a test case. Further, the extent of change in both variables makes late 19th-century France appropriate for studying the relationship between policing and crime.

The specific focus of this analysis is the growth of national policing and

its effect on rates of major and minor crimes in France between 1865 and 1913. This period is also appropriate since the penal code was revised in 1863 for the last time in the 19th century, reducing some serious crimes to misdemeanors and further restricting the discretionary power of judges (Wright 1983), and 1914 brought the disruption of World War I. Restricting the analysis to this period, then, decreases the likelihood that any patterns observed in the data simply reflect changes in the law.

Although criminal statistics must be approached with care, even when they are recently compiled, "Frenchmen in the nineteenth century did possess one of the world's best statistical records of that era for making educated guesses about trends in crime" (Wright 1983, pp. 96–97). Criminal statistics were published annually, from 1826 on, in the *Compte générale de l'administration de la justice criminelle*.

French population statistics for the 19th century are also relatively high in quality. Because France led the decline in European fertility, French population statistics receive a great deal of attention from historical demographers, so limitations are known and correction factors have been developed. For the most part, French census enumerations, first published in 1851, converge convincingly with other estimates of population characteristics, especially since 1856 (see, e.g., Van de Walle 1974).

The French population gradually escaped what Marx called "the idiocy of rural life" by abandoning, or being forced from, the land and agricultural occupations and joining the industrial proletariat in urban areas (see McQuillan 1984). During the 19th century, the number of people living in urban areas in France almost tripled. In 1821, urban France contained just over six million, which was about 20% of the total population. By the end of the century, almost 16 million (15,957,000 in 1901) lived in urban areas, 41.0% of the population, and by the beginning of the war, about 45%. The urbanization of France proceeded in a generally linear fashion over the 19th and early 20th centuries except for a slight depression during the war in 1870. (The old definition of urban area, a settlement of at least 2,000 people, is used for all years here.)

The second half of the 19th and the early part of the 20th centuries were an era of widespread technical, economic, and demographic change in France. Although there was only one major war (the Franco-Prussian War, 1870–71), the age can hardly be described as peaceful. It was a period of intense and extensive social challenge and conflict, which occurred for the most part within the country, pitting an emerging proletariat against its employers and, increasingly, the state: "Sometime in the nineteenth century the people of France shed the collective-action repertoire they had been using for about two centuries and adopted the repertoire they still use today. A definitive shift to the new repertoire did not become complete until the 1850s" (Tilly 1986, p. 391).

Crime

The new repertoire supplanted traditional and parochial forms of collective protest, such as seizures of food (food riots), serenades, and charivaris, which were usually directed against local patrons. Strikes, mass meetings, petitions, demonstrations, and social movements became the new ways of protesting, and these typically challenged wider authority rather than local patrons (Tilly 1986). Further, urban industrial societies were more sensitive to these new forms of protest (Polanyi 1944), so they were often effective. Throughout the 19th century, and particularly during the fin de siècle, elites perceived that the nation was in decay and that their class position was weakening (see Weber 1986). Perhaps more in response to this, then, and in the interest of maintaining order and the status quo, the nature and extent of state control also changed with the advent of national policing. (See, esp., Lyman 1964; Lane 1967; Silver 1967; Monkkonen 1975; and see Black 1976; Cobban 1961, 1965; Mawgraw 1983; Tilly 1986; Weber 1976, 1986; and Wright 1981 for more general treatments of this period of French history.)

The Expansion of State Surveillance

State policing began in France early in the 19th century. From well before the Revolution, an elite regiment of the army, the *Maréchaussée* (from which "marshal" is derived), had served as the military police in rural areas and later as Napoleon's bodyguard as well. During the 19th century, the military duties of this regiment were greatly eroded as policing became more and more its *raison d'être*. These *gens d'armes* (men at arms) became the *Gendarmerie nationale*, the national police force of France. They still patrol the countryside, highways, and towns of less than 10,000 in population, act as the bodyguard of the president, and continue to be administered as a regiment of the French army by the Ministry of Defense in Paris (see Cameron 1977; Stead 1984; Tilly et al. 1975; Wright 1983; and Emsley 1983).

The task of policing the larger towns and cities of France fell to the *Sûreté*, a national civilian police force administered by the Ministry of the Interior in Paris. In 1829, several months before Peel's bobbies began patrolling the streets of London, 100 uniformed officers appeared on the streets of Paris. Giddens (1985, p. 230) notes that uniforms indicated to civilians that the wearer was a "specialist purveyor of the means of violence." According to Stead (1984), the uniform also kept "in the public's mind the presence of policemen" and encouraged the public to assist the police when the police needed help. The prefect of police, Debelleyme, also hoped uniforms would discourage officers from frequenting taverns and openly engaging in "bad habits," such as intemperance and gam-

bling, and would discourage policemen from vanishing into the crowd when trouble started (Stead 1984, pp. 54-55).

Both national police forces grew throughout the 19th century, with the *Sûreté* (now the *Police nationale*) consistently increasing with urbanization. The size of the *Police nationale* eventually exceeded that of the *Gendarmerie*, the growth rate of which had leveled off by 1880. The *Gendarmerie nationale* now numbers over 80,000 with the *Police nationale* at 108,000. In view of the size of the French population (about 52 million), the ratio of police to public is relatively high in comparison with other Western countries, such as Britain or the United States.

The growth of state policing in France involved far more than a specific focus on crime control. Tilly (1986, p. 36) notes that the growth of national policing brought three important changes: "First, the surveillance, control, and repression of popular collective action became the business of the national government's specialized local representatives: police, prosecutors, spies, and others. Second, the procedures of surveillance, control, and repression, bureaucratized and routinized, became objects of regular reporting and inspection. Third, anticipatory surveillance increased greatly: authorities watched groups carefully to see what collective action they might take in the future, and be ready for it."

Courts and Crime Rates

There were three court systems in France during the 19th century. The *cours d'assises* dealt with *crimes* (serious offenses such as index crimes or felonies in the United States). The *tribunaux correctionnels* adjudicated *délits* (minor offenses, corresponding to misdemeanors). The *justice de paix* was concerned with *contraventions*—petty violations such as traffic, hunting, and customs offenses, which were often policed by *gardes champêtres*, who were "a cross between a rustic policeman and a forest ranger" (Stead 1984, p. 9). *Contraventions* were heard by justices of the peace, who could impose fines or prison sentences up to two years (see also Cohen and Johnson 1982; Tilly et al. 1975; Wright 1983).

Research on patterns of crime during the 19th century converges in some respects and diverges in others. Some studies suggest that rates of serious personal violence were higher in rural than in urban areas and declined during the second half of the 19th century, while property crime was more an urban offense and increased over the same period (see Cohen and Johnson 1982; Lodhi and Tilly 1973; Weber 1976; Zehr 1975; and, more generally, Fischer 1980). However, these studies were for the most part cross-sectional analyses or relatively confined in the periods on which

they focused. Longitudinal research shows that rates of serious crime declined while rates of minor offenses increased over the second half of the 19th and first half of the 20th centuries (Davidovitch 1961; also Chesnais 1976, 1981). So researchers agree that rates of serious violence declined and that rates of minor property crime increased but disagree on whether rates of serious property crimes and minor acts of violence increased or decreased during the 19th century. In view of this, it is empirically as well as theoretically important to distinguish major from minor crimes of violence (e.g., homicide or rape vs. common assault), as well as major from minor property crimes (e.g., robbery vs. petty theft).

I examine the relationship between the expansion of state control, as reflected by the changes in formal policing, and the changes in crime in France during the 48-year period between 1865 and 1913. The analysis focuses on charges for *crimes* and *délits* that resulted in appearances before the *cours d'assises* and the *tribunaux correctionnels*, respectively. The study concentrates on charges for both serious and minor offenses but excludes petty violations. Data are transcribed from the *Compte général de l'administration de la justice criminelle en France*, in the *Ministère de la justice*, and the *Annuaire statistique*, in the *Bibliothèque nationale*, both in Paris.

In order to control for the effects of changes in population, I express the units for the analysis as rates. The dependent variables are rates of major and minor crimes per unit population, and the independent variables are number of gendarmes and number of police (*Sûreté*) per unit population. This makes sense theoretically and avoids possible problems arising from analyzing the relationship between variables expressed as rates and others that are not (see Firebaugh and Gibbs 1985, 1987; Bradshaw and Radbill 1987; and Kraft 1987, for an extensive discussion of this issue).

ANALYSIS

The growth of the two police forces has been described, so the analysis will begin here by describing the changing rates of crime. During the period from before the Franco-Prussian War in 1870 to the beginning of World War I, rates declined for both types of major crime (figs. 1 and 2). (An extended examination of these rates shows that the trends continued through to the early 1960s.)

In the *tribunaux correctionnels*, rates for minor offenses against persons steadily increased throughout the period (fig. 3), but minor property offenses rose sharply for the first 30 years and then declined just as sharply from the turn of the century until just before the war (fig. 4).

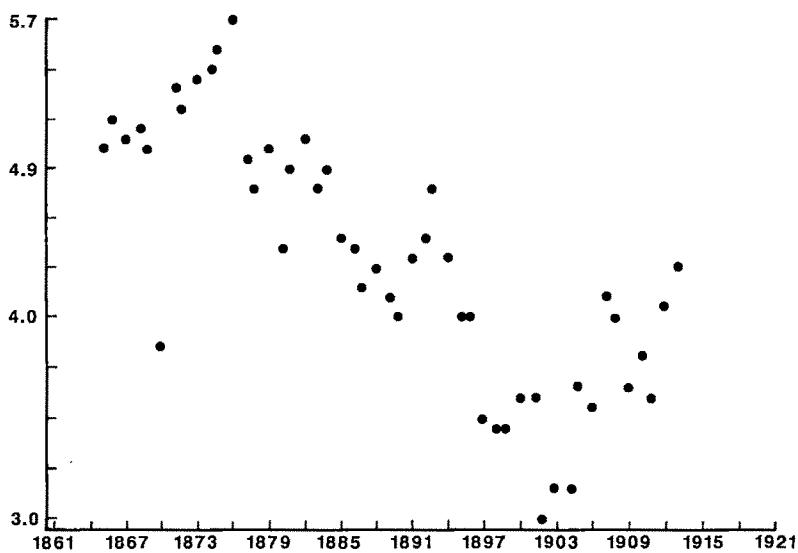


FIG. 1.—Major crimes of violence, charges per 100,000

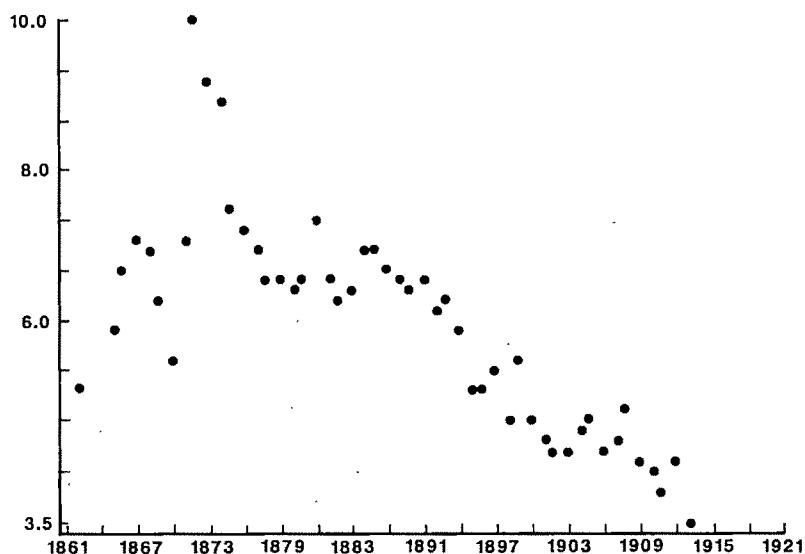


FIG. 2.—Major property crimes, charges per 100,000

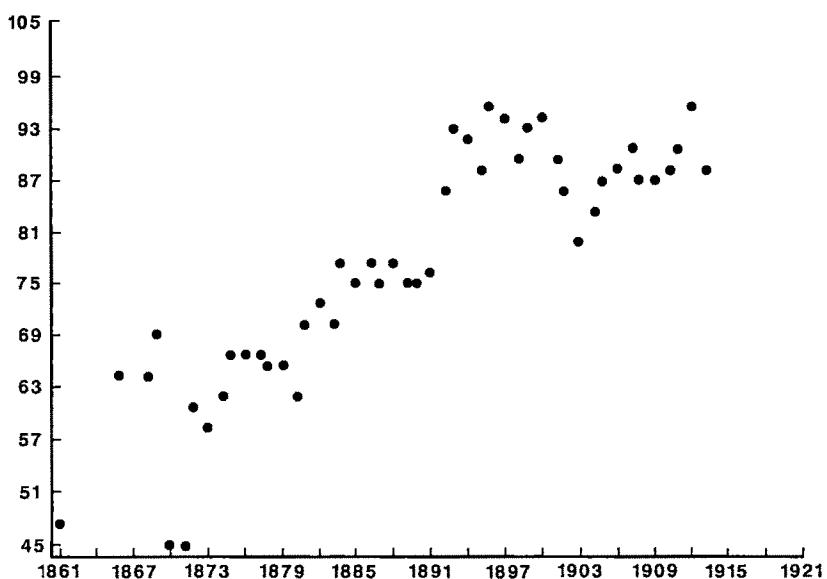


FIG. 3.—Minor crimes of violence, charges per 100,000

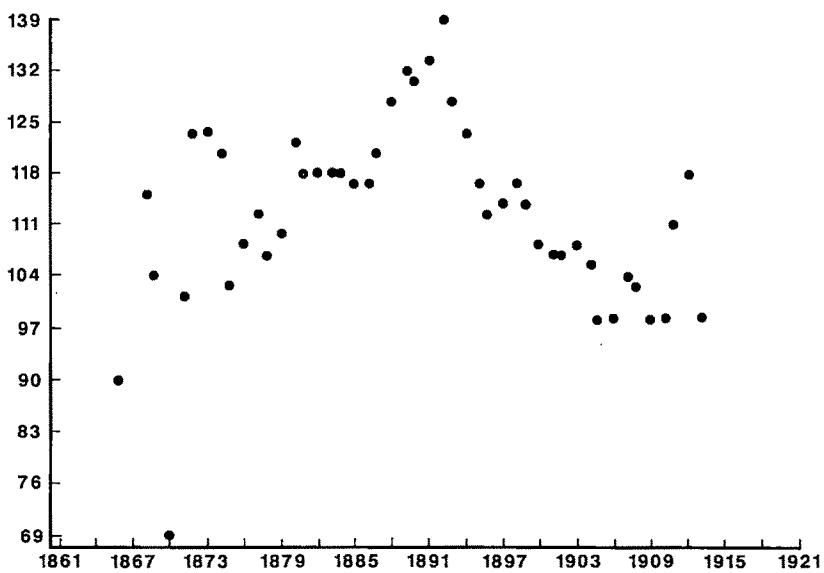


FIG. 4.—Minor property crimes, charges per 100,000

American Journal of Sociology

(Following World War I, the rates for minor property offenses rose sharply, dropped just before World War II, and then rose again into the 1950s.)

All crime rates fell during the Franco-Prussian War but rebounded during the destruction of the Paris Commune in 1871, and in the years immediately following, to settle back finally on their prewar paths (see Lodhi and Tilly 1973). (Similar effects of war on crime have been observed elsewhere [see Archer and Gartner 1984].)

Autocorrelation and the Analysis of Contemporaneous Effects

The zero-order correlations between time and these rates of crime are strong. However, the coefficients overstate the linear fit of time with the different rates of crime. Analyses of temporally ordered data are more likely than others to be distorted by autocorrelation, a condition where an error term and its own lag are correlated. When the adjacent case is a better predictor of a specific case than is the mean, an important assumption of ordinary least-squares (OLS) regression is violated, and slopes may be inflated and difficult to interpret. In the following analysis, bureaucratic inertia could have an effect on the size of police forces, so the best predictor of the number of police officers in a specific year is the number in the preceding year. Also, contagion and inertia could make the crime rate in a specific year the best predictor of the crime rate in the following year. This situation would confound attempts with OLS to interpret slopes for rates of crime on policing. Therefore, whenever the standard Durbin-Watson *d*-statistic indicates the presence of autocorrelation, a version of generalized least-squares (GLS) regression (the Hildreth-Lu transformation) is used instead of OLS regression (see Johnston 1984; Kmenta 1971; Ostrom 1978). After I used GLS, the Durbin-Watson *d*-statistic was around 2.0 for all equations, indicating that this procedure had largely eliminated autocorrelation.

The zero-order correlations with no lag show that there is a direct relationship between the size of both police forces and the rates of major and minor crimes of violence. For the gendarmes, the correlations are $r = .35$ and $r = .77$ for major and minor crime rates, respectively, and for the police these values are $r = .57$ and $r = .55$. These coefficients suggest that the growth of the state inflated rather than deflated rates of violence. The increasing crime rate may have resulted in the growth of policing, or, as noted earlier, the growth in policing may have resulted in more charges. The latter seems more likely because the response of states to an increasing crime rate is unlikely to have been so immediate. However, either or both may contribute to the observed correlations.

Distributed Lags: Crimes of Violence

If a deterrent effect from increased policing offset an initial inflationary effect, it could show up in an equation with distributed lags. Here the dependent variable is regressed on different observations of the independent variables at an earlier period. These different observations are determined by different years, beginning with the present and extending back four years. That is, the size of the police force as well as its size in each of the preceding four years can be assessed as predictors of crime (see Dhrymes 1971; Stewart 1984). Contemporaneous effects show up with zero lag (t), and long-run effects become apparent when the value of an exogenous variable for an earlier period ($t - n$) is introduced in its lagged form as another exogenous variable.

Exogenous rather than endogenous variables are being lagged; therefore, estimation problems are confined to the possibility of multicollinearity and reductions in cases and degrees of freedom (Stewart 1984). As it turns out, none of these possibilities causes difficulties. Multicollinearity is minimal, and the period of analysis is sufficiently long so that degrees of freedom remain well above the accepted cutoff point of 30 (Ostrom 1978), even with a five-year focus (no lag, and lags for each of the preceding four years). (The extension of the lag period does not add explanatory power to any equations and reduces adjusted r^2 's. In view of this, confining the focus is justifiable on empirical grounds.)

The intercorrelations among the different values of the independent variable do not produce any problems of multicollinearity. The largest single correlation coefficient is $r = -.36$ for the police, lagged zero and one years as a predictor of rates of major offenses. (These correlation coefficients are listed in single columns headed by a zero. It is unnecessary to show all possible intercorrelations since the correlation between lags zero and one, e.g., will be the same for lags one and two, lags two and three, etc. [see table 1].)

The distributed lags initially provide little support for an argument that the growth of policing repressed serious crimes against persons. It is clear in all instances that any contemporaneous effect of increased policing is inflationary, especially in the case of minor offenses. The betas for the gendarmes and the police on serious crimes are $\beta = .40$ and $\beta = .47$, respectively, and for minor offenses these values are $\beta = .76$ and $\beta = .70$ (table 1). Although the long-term effect of policing seems to have been deflationary, these deterrent effects are weaker than the initial inflationary effect and cannot account for the declining rates of serious crimes of violence.

In the case of minor offenses, the immediate inflationary effect of policing is dramatically greater than any lagged repressive effects. For the

TABLE 1
VIOLENT CRIME

	0	r	b	SE	β	β_1	β_2
Gendarmes (rural):							
Major:							
Lag in years:							
0.....	1.00	.35**	.012	.004	.40**	.29	.44**
1.....	.01	-.38**	-.013	.004	-.43**	-.41*	-.18
2.....	-.19	.05	.005	.004	.15	.18	.16
3.....	.07	-.08	-.006	.004	-.19	-.20	-.07
4.....	.00	.00	.002	.004	.07	.09	.01
Minor/major crime							
Urbanization							
r^2 (adjusted)					.24	.26	.31
Minor:							
Lag in years:							
0.....	1.00	1.03E-04	1.60E-05	.70***	.70***	.58**	.78***
1.....	-.32**	8.37E-05	1.67E-05	.57***	.57***	.65**	.70***
2.....	-.03	6.08E-05	1.66E-05	.04	.10	.20	
3.....	.10	.242E-05	1.64E-05	.17	.22	.30	
4.....	.13	.1.39E-05	1.57E-05	.10	.13	.18	
Minor/major crime							
Urbanization							
r^2 (adjusted)					.57	.60	.57

Police (urban):

	Major:	Police (urban):			
	Lag in years:	.57**	.005	.47*	.17
0	1.00	-.57**	-.005	.47*	.61*
1	-.36*	-.40**	-.003	-.29*	.22
2	-.06	-.04	-.002	-.17	.22
3	-.07	-.01	-.001	-.15	.19
4	-.09	-.03	-.001	-.11	.07
Minor/major crime					.43
Urbanization					-.72***
r^2 (adjusted)					.37
Minor:					
	Lag in years:				
0	1.00	.77***	3.35E-04	4.56E-05	.76***
1	-.03	-.10	-1.73E-05	4.51E-05	-.04
2	-.25*	-.19*	-1.64E-05	4.68E-05	.00
3	.03	.17	5.99E-05	4.50E-05	.14
4	-.04	-.08	-2.12E-05	4.53E-05	-.05
Minor/major crime					.21**
Urbanization					.15
r^2 (adjusted)					.57

NOTE.—Intercorrelations between lagged years (0), zero-order correlations (ρ), unstandardized partial slopes (b), standard errors (SE), and standardized partial slopes (β) for rates of visible crimes of violence by gendarmes and by police per 1,000 population, controlling for rates of major and minor crimes (β_1) and urbanization (β_2); distributed lag models for France, 1865-1913. Major, $N = 48$; minor, $N = 45$.

* $P \leq .05$.

** $P \leq .01$.

*** $P \leq .001$.

gendarmes, the initial slope is $\beta = .76$, whereas the largest following effect is $\beta = -.14$. The inflationary effects are even more dramatic for the urban police, where a slope of $\beta = .70$ is followed by a large inflationary effect the following year of $\beta = .57$.

As noted earlier, there were no important changes in the law itself during the period under analysis. However, a gradual move toward leniency may have produced a shift in patterns of charges, where offenders were more likely to be charged for minor offenses before the *tribunaux correctionnels* than serious crimes before the *cours d'assises*. To control for this possibility, I include the rate of minor offenses in the equations predicting the rate of major crimes of violence. If there was a shift in charging major offenders with minor crimes, it should show up in the form of a negative correlation between the two rates with a zero lag, and the effect would be eliminated. In the equations predicting rates for minor offenses, the rates for major crimes of violence are the control variables. The results appear in the columns headed by β_1 .

The data do not suggest that there was an increasing tendency to charge major offenders with minor crimes. The relationship between rates of major and minor offenses is statistically significant (zero-order correlations around $r = .40$) but is positive, not negative. In any given year, then, an increase in charges for major property crimes was directly related to an increase in charges for minor offenses. This suggests either that major and minor criminal activities tend to increase or decrease simultaneously or that changes in police behavior initially affect charges for both major and minor crimes in the same way. However, since rates for minor and major crimes of violence were rising and falling, respectively, the former seems less likely than the latter. So the zero-order relationship between rates of major and minor offenses is probably based primarily on their joint dependence on policing.

Because they are so strongly correlated with each other, and because the pattern of the distributed lag is different for each dependent variable's relationship with the gendarmes and the police, major and minor rates of crime could distort the slope for each other on policing. Including these variables as controls, then, is potentially important. For major crimes of violence, the immediate inflationary effect of the gendarmes and the police is reduced from $\beta = .40$ to $\beta_1 = .29$ for the gendarmes, and from $\beta = .47$ to $\beta_1 = .17$ for the police. For the police, the deflationary effect after one year also grows stronger, from $\beta = -.29$ to $\beta_1 = -.54$. Controlling for rates of major crimes against persons has little influence on the effect of policing on charges for minor offenses. In general, then, although there is support for the idea that the rate of minor crime may influence the immediate inflationary effect of policing on rates of serious crime, the data do not support the argument that the decline in official

Crime

rates of serious crime was caused by an increasing tendency to charge offenders with minor crimes.

Urbanization

In view of the argument that cultural changes associated with urbanization increased the nonviolent settlement of disputes, urbanization (percentage of the population classified as urban) was included in all equations as a control variable (col. β_2). For major crimes of violence, the effects of this are dramatic. Urbanization is the strongest predictor ($\beta_2 = -.45$ and $\beta_2 = -.72$, respectively) in the equations involving both the gendarmes and the police. Further, the immediate inflationary effect of policing is substantially increased, especially for the police, where $\beta_2 = .61$, and the subsequent deflationary effects of policing are eliminated or reversed. Including urbanization in the equations for rates of minor offenses produces no significant changes.

Property Crime

The initial patterns for crimes of violence displayed in table 1 emerge even more dramatically in the analysis of the effect of policing on serious property crime. Contemporaneous inflationary effects of policing on serious offenses are weaker, and delayed deterrent effects are larger (see table 2).

In the case of minor property crime, the inflationary effect on charges is somewhat less dramatic than it was with minor offenses against persons. Nevertheless, the immediate effect of the urban police on minor property crimes was considerable, $\beta_1 = .66$. The largest subsequent deflationary effect is $\beta_1 = -.24$, with a three-year lag, and none is statistically significant. So the overall effect of the expansion of policing on rates of minor property crimes is inflationary.

Introducing urbanization into the equations for property crimes does not substantially alter the earlier patterns. The initial effect of both types of policing on serious offenses continues to be positive and moderate in size. Lagged effects are negative and stronger, especially in the case of the police, where an immediate effect of $\beta_2 = .25$ is followed by $\beta_2 = -.09$, $\beta_2 = -.72$, $\beta_2 = -.35$, and $\beta_2 = -.32$ in successive years. This suggests that subsequent deflationary effects outweigh the initial inflationary one and that policing may have indeed been a cause of the decline in serious property crime, independent of urbanization. For minor property offenses, controlling for urbanization has almost no effect on the initial betas, which are large and relatively stable. It is noteworthy that when the police are the exogenous variable, the lagged betas suggest the

TABLE 2
PROPERTY CRIME

	0	r	b	SE	β	β_1	β_2
Gendarmes (rural):							
Major:							
Lag in years:							
0.....	1.00	.46**	.017	.006	.31**	.08	.32**
1.....	-.04	-.24*	-.014	.006	-.26*	-.12	-.24*
2.....	-.24*	-.56***	-.031	.006	-.56***	-.53***	-.53***
3.....	.04	-.01	-.006	.006	-.11	.04	-.09
4.....	-.03	-.17*	-.016	.006	-.29**	-.32*	-.27***
Minor/major crime...							
Urbanization							
r^2 (adjusted).....				.52	.64	.20	.55
Minor:							
Lag in years:							
0.....	1.00	5.67***	4.66E-04	9.40E-05	6.1***	.35***	.61***
1.....	.09	-.17*	-2.06E-04	9.32E-05	-.27*	-.17	-.26
2.....	-.12	-.15	-3.01E-04	9.33E-05	-.01	.23	.01
3.....	.11	-.17*	-2.08E-04	9.25E-05	-.27*	-.27*	-.26
4.....	-.04	.10	1.08E-04	9.14E-05	.14	.22	.16
Minor/major crime...							
Urbanization							
r^2 (adjusted).....					.37	.54	.36

Major:	
Lag in years:	
0.....	.10
1.....	-.10
2.....	-.66***
3.....	-.14
4.....	-.29*
Minor/major crime19
Urbanization	
r^2 (adjusted)	
Minor:	
Lag in years:	
0.....	1.00
1.....	-.42**
2.....	-.10
3.....	.04
4.....	.06
Minor/major crime	
Urbanization	
r^2 (adjusted)	

NOTE.—Intercorrelations between lagged years (0), zero-order correlations (r), unstandardized partial slopes (b), standard errors (SE), and standardized partial slopes (β) for rates for visible property crime by gendarmes and by police per 1,000 population, controlling for rates of major and minor crimes (β_1) and urbanization (β_2); distributed lag models for France, 1865-1913. Major, $N = 48$; minor, $N = 45$.

* $P \leq .05$.

** $P \leq .01$.

*** $P \leq .001$.

American Journal of Sociology

possibility of subsequent deterrent effects ($\beta_1 = -.03$, $\beta_2 = -.45$, $\beta_3 = -.38$, and $\beta_4 = -.31$). However, even in combination, they do not compensate for the initial inflationary effect of policing ($\beta_5 = .69$).

Predicting Policing

The main purpose of this analysis is to examine the effect of state surveillance on crime. However, in view of earlier arguments, it is also important to examine the effect of changing crime rates on the growth of policing. If the expansion of state surveillance were merely a response to apparent changes in patterns of crime (particularly charges for minor offenses, which were increasing and over which the police themselves had a great deal of control), the argument tying policing to the centralization of the power of the state would seem overdrawn. The effect of crime on policing, then, has important implications.

Severe reductions in cases and degrees of freedom, and other factors, discourage examining lagged values of the endogenous and exogenous variables simultaneously (see Johnston 1984; Ostrom 1978; Stewart 1984). In view of this, the equations shown in tables 1 and 2 were reversed, with the gendarmes and the police as the dependent variables and current and lagged rates of major and minor crimes as the predictors. Again, lagging the exogenous variable posed no serious problems. However, apart from the relationships between crime rates and the rates of gendarmes and police per 1,000 population for the same year, of which I was already aware, the results were not noteworthy. In only one instance (the effect of the rate of major offenses one year earlier on the number of gendarmes, $\beta_1 = .35$) were rates of visible crime significant predictors of changes in police or gendarmes.

DISCUSSION

This analysis shows that rates of serious crime declined in France over the second half of the 19th and the early part of the 20th centuries. This decline was as clear for major property crimes as it was for crimes of violence. Over the same period, rates of minor offenses generally increased, though the increase in minor acts of violence was more consistent and dramatic than was the growth of minor property offenses.

The growth of state policing in France between 1855 and 1913 may have had an important effect on these changes. The most immediate seems very likely to have been an increase in the number and rate of arrests and charges, particularly for minor offenses. But for major crimes involving violence and, especially, property, initial increases are more

Crime

than offset by subsequent decreases within two, three, or four years. Including urbanization in the equations as a control variable eliminates this effect for major crimes of violence, but the relationship between policing and rates of major property crimes remains unchanged. The fact that both major and minor property crimes seem to have been more sensitive to policing than violent crime, which is more spontaneous in its nature, is consistent with deterrence/repression theory.

The essence of deterrence theory is punishment. When it is sufficiently severe, certain, and swift, punishment is supposed to counter the rewards of crime and, at least in rational people, repress criminal behavior. Certainty has more general deterrence value than severity (see Tittle 1969; Chiricos and Waldo 1970; Bailey, Martin, and Gray 1972; and for a recent summary, Ross 1986), but these elements of deterrence theory are essentially nonadditive. Logically, neither certainty without sanctions nor sanctions without certainty deter, so a measure of both is necessary. In earlier periods, when nation-states were weak and poorly organized, the certainty of punishment was probably low. Although by present standards exemplary punishment was typically severe, current research suggests that the general deterrence value would also have been low. However, with the expansion of state policing, the certainty of punishment probably increased, giving it a greater capacity to deter, especially for crimes that continued to carry severe punishment. Although the severity of punishment was in general decline during the 19th century (O'Brien 1982; Wright 1983), juries in France continued to give relatively severe sentences to property criminals (Donovan 1981). In view of this, it is not surprising that policing had its greatest effect on rates of major property crimes. For minor property offenses, the effect of increased certainty may have been more than offset by declining severity. (It is noteworthy that urban policing seems eventually to have had a significant deterrent effect on minor property crime, although it was less than the immediate inflationary effect.)

Before policing is dismissed as irrelevant in explaining the decline of serious violence, the relationship between urbanization and policing ought to be closely examined. The expansion of the *Sûreté* was in part determined by urbanization, and the growth of both police forces is strongly correlated with it. So controlling for urbanization may obscure the power of policing as an intervening variable through the attenuation of its variance. However, since this did not happen for property crime, the failure of policing to persist as a predictor of rates of serious violence casts doubt on the importance of state surveillance even as an intervening variable. The nature of violent crime may simply make it more difficult than property offenses to repress. A follow-up investigation currently

American Journal of Sociology

being conducted on more specific categories of crime in various regions of France with wide differences in the level and nature of urbanization should shed more light on these possibilities.

The failure of policing to deter major crimes of violence when urbanization is in the equation could result from several factors. One is that urbanization produced or reflected the civilizing changes in cultural patterns described earlier and that this reduced serious crimes of violence and generated growth in the police force as well. As also noted earlier, increases in population density associated with urbanization may have amplified informal public surveillance, and this deterred serious violence or prevented minor incidents from becoming more serious (see Jacobs 1961; Stinchcombe 1963; Hagan, Gillis, and Chan 1979; Gillis and Hagan 1982; Sampson 1986; Smith 1986; Sampson and Groves 1988).

Although these explanations are plausible, the relationship between urbanization, civility, crime rates, and these other factors ought to be examined empirically with more direct indicators before drawing any conclusions. Urbanization is associated with a number of variables, including industrialization and the emergence of corporate capitalism, and these may have made the real contributions to changing rates of crime in 19th-century France. Indicators of these arguments should also be included as alternative predictors.

Several factors may have contributed to an increase in rates of minor criminal behavior. Industrialization produced more items to steal, and urbanization may have produced more opportunities to steal them (see Cohen and Felson 1979; Messner and Blau 1987). Declining employment may have combined with this to tip the balance still further in favor of larceny. The increasing size, density, and social heterogeneity of populations may have provided not only greater opportunity for people to become involved in minor altercations but also the urges to do so (see Milgram 1970; Simmel 1970; Wirth 1938). An increase in the use of alcohol and morphine following the Franco-Prussian War (Weber 1986) may have exacerbated the situation.

Although these factors may have produced increases in minor offense rates, increases in arrests and charges may also have occurred independent of any actual change in rates of minor criminal behavior. As noted earlier, the increasing concentration of the population in cities combined with the growth of public space to reduce privacy, and most behaviors, legal and illegal, became more visible. These conditions could result in an increasing rate of arrests and charges without any real increase in frequency of infractions. Research in other places and periods suggests that rates of minor offenses are highly susceptible to changes in police organization, procedures, and discretion (see, e.g., Boritch and Hagan 1987; McCleary, Nienstedt, and Erven 1982; Monkkonen 1981; Slovak 1986).

The same pattern seems to have held in France during the 19th century. Adolph Quetelet ([1842] 1969) observed that it is very likely that for major crimes, official data are more reliable measures of criminal activity than for minor offenses, which to a much greater extent reflect other variables in the judicial process, including the activities of the police (see Beirne [1987] on Quetelet). The fact that beta with zero lag is much greater for policing as a predictor of minor rather than major offenses supports this, as does the fact that controlling for minor offense rates greatly reduces the immediate effect of policing on rates of serious crime.

Because of multicollinearity, it is impossible to determine in one equation whether the *Sûreté* or the rural *Gendarmerie* had the greater effect on changing rates of visible crime. Both inflationary and deflationary effects are stronger in the equations for the police than for the gendarmes, suggesting that the effect of the former was greater. But the differences are not large enough to enable us to conclude that urbanization in combination with the growth of policing produced the changes in crime rates between 1865 and 1913. The greater predictive power of the urban police may derive from their greater numbers and variance rather than the greater concentration of their activities in urban areas. Further, rates of serious crime were declining during this period in rural France, and the presence of the judicial system was increasingly evident (see Weber 1976, pp. 51–66; also LeGoff and Sutherland 1974). In view of this, and the relationship between urbanization, policing, and serious crimes of violence, the rural-urban question is still open and should be addressed by research combining cross-sectional and longitudinal approaches, such as pooled time-series analysis.

Except for contemporaneous effects, crime rates are generally poor predictors of policing, except in the zero-lag situation. This could mean that crime rates had an immediate effect on the growth of the police. However, as noted earlier, this is unlikely. The information that an increase in crime had occurred would not be immediately available, and even if it were, it seems unlikely that government bureaucracy could respond instantly. It is more likely that the positive relationships between policing and crime in the same year reflect an increase (or decrease) in charges owing to an increase (or decrease) in the number and ratio of officers to the population.

It is also possible that the effects of changing crime rates on policing are not felt until much later. If this were the case, the four-year lag would reveal nothing. However, this may stretch things too far in the other direction. Government bureaucracy is slow, but it is unlikely to have been that slow. To increase policing five years or more after changes in crime rates seems implausible. Even when annual budget allocations for policing were substituted as the dependent variable, there was no im-

provement in the predictive power of the arrest rate for major or minor crimes. (There was also no relationship between changes in the budget and policing for the current year, further supporting the idea that immediate increases in charges result from increased policing, rather than the other way around.) Nevertheless, the possibility of a long-term effect from an early period cannot be eliminated. A "sharp burst" of crime early in the 19th century (Wright 1983, p. 49) may have set in motion a growing concern with crime and criminals, which had an important long-term influence on the arts (e.g., Victor Hugo's *Les Misérables*, published in 1862), and the widespread public fear of criminality, which persisted throughout the 19th century (see Wright 1983; Beirne 1987).

Immediate factors may also have increased concern about crime even though rates of serious crime were in decline. Prevailing beliefs in France during the fin de siècle were contradictory in several respects. For example, the general standard of living was improving, as was the political power of the masses, yet the period was associated with "decadence," especially in the minds of those in the ruling classes (Weber 1986). It is possible that the gains of the lower classes produced a sense of erosion and decay in the ruling classes. It is also possible that the pessimism of the literate populations, particularly in Paris, was produced by the increasing visibility of crime and corruption. In spite of declining rates of serious crime, the concentration of populations in urban areas probably made criminal behavior more visible. This would have given the impression of increasing crime, regardless of whether the population was growing faster (particularly in the urban areas) and pulling the rates down. Further, the emergence and proliferation of sensationalist newspapers, such as the inexpensive *Petit Journal* (1863), made decadence a daily concern of those who could read (Weber 1986). Ironically, the appearance of these French forerunners of the *National Enquirer* probably inspired and enabled many people to become literate (see Weber 1976; also Furet and Ozouf 1977). In any case, the apparent obsession of the literate classes with decadence in urban areas is understandable even though rates of serious offenses were declining.

Chevalier (1973), Rudé (1964), Donovan (1981), and others note that early in the 19th century the idea of "dangerous classes" emerged (see Frégier 1840) and became a popular explanation for criminality, along with the miasmic theory of disease, which postulated that illness arose from decaying organic material: "Crime was a social disease, moral pollution no more a private concern than the pollution of water or air that threatened the well-being of all. As the nineteenth century advanced, social disorders more and more came to be interpreted as maladies, connected with physical infections in a pathology of corruption that seemed

Crime

as evident in urban decay as in the moral and physical degeneration of the lower classes and of their even more visible betters" (Weber 1986, p. 42).

Like the ideas of "social pathology" and "social disorganization," which were to come later, the idea of dangerous classes obscured distinctions between common crime and political protest. (It is noteworthy that, by classifying all lawbreakers as reformers and revolutionaries, "new" urban sociologists also reject these distinctions [see Zukin 1980].) In any case, the idea of dangerous classes may have combined with the new, more effective repertoires of social protest to increase public concern about the fabric of society itself.

The origin and growth of state policing in France may have always derived from an interest on the part of whatever elite was ruling in protecting itself and the status quo rather than from a general interest in crime control. (Even during and following the Revolution in 1789, common criminals were released from prisons to make room for political opponents.) Lane (1974, 1980), Silver (1967), and others argue that threats to public order (especially riots), more than an interest in crime prevention, precipitated the development of police forces in a number of locations. In France, the security of the state was always an essential element of policing. In comparing England and France, Emsley (1983) notes that "prevention" described the orientation of the English police force; in France, "surveillance" characterized the orientation of the police, and the basis for surveillance was frequently the maintenance of the social and political order more than crime control.

Several prefects in General Duverger's district of northeast France had asked for an increase in *gendarmes*, and Duverger supported their requests for reasons which had nothing to do with fears of increasing crime: Boulogne needed more than one brigade because it was a populous town with many travellers passing through; the district around St Pol needed a fourth brigade simply to maintain *la surveillance* over its eighty-one communes; Dormans required an extra man, preferably an NCO to supervise the large number of workers attracted to the area. [Emsley 1983, p. 86]

In line with this, Tilly et al. observe that between 1825 and 1900 "the five years which brought the largest increases in the national expenditure for policing were 1831, 1852, 1860, 1868, and 1872. Three of them immediately followed a revolution or coup. The other two came in the aftermath of major threats to Louis Napoleon's regime" (Tilly et al. 1975, p. 88). Stead goes so far as to conclude that "much more importance was attached to keeping Napoléon III in power than to the basic purposes of police work. The police even provoked riots so that the imperial government could have the credit for putting them down" (Stead 1984, p. 69).

Whether policing in France actually repressed legitimate (or illegiti-

American Journal of Sociology

mate) political action is uncertain. Lord Blayney, traveling through France early in the 19th century, wrote that "the gend'armerie forms the most efficient military police in Europe, and is so well-established, that not only the roads are safe, but the people are also kept in complete political subjection" (Emsley 1983, p. 40). Lord Blayney was an English prisoner of war, and his objectivity is as questionable as his access to data, but in view of everything else, his observations suggest that the issue deserves further attention.

CONCLUSIONS

Structural change, in the form of the development and expansion of state surveillance, may have helped to produce much of the decline in serious property crime during the second half of the 19th and first part of the 20th centuries in France. The rational nature of property crime makes it susceptible to deterrence, and a continuing high level of severity may have combined with the increasing certainty of punishment to create the ideal conditions for effective repression. Since property offenses were the clear majority of all serious offenses, the contribution of policing to the overall decline of serious crime would have been substantial. However, if anything, state policing impaired rather than contributed to a continuing decline in crimes of violence, especially minor offenses.

The findings of this analysis are consistent with deterrence theory and the research that supports it. It is noteworthy that many social scientists are ambivalent about deterrence theory. This seems particularly true of Marxists. Jack Gibbs (1978) observes that, although Marxist theory assumes that coercion and repression by the state is effective, there is a disinclination among Marxists even to use the term "deterrence," let alone acknowledge its efficacy. He calls this "the secret scandal" of Marxist theories of criminal law (see Gibbs 1978, pp. 106-7).

The reason for this ambivalence is probably a well-founded suspicion that political and economic repression is justified with concepts such as dangerous classes and the need for law, order, and deterrence. Turk (1982) notes that vague laws and blurred distinctions facilitate the prosecution of political crimes, and the situation in 19th-century France supports such skepticism. In spite of the rhetoric about the control of crime, crime rates' inability to predict changes in policing undermines the idea that deterrence of crime was the intended result of increased surveillance. Further, because political threat immediately preceded expanded surveillance, the state's interpretation of "dangerous" may have been affected more by political than criminal acts, so repression seems at least as likely a motive as deterrence. However, whether intended or not, increased

surveillance by the state is associated with declining rates of serious property crime. If the expansion of national policing discouraged rational political as well as rational criminal activity, the reduction of serious property crime may have been purchased at an excessive cost, especially for those who were not owners of much property. This is a matter of value. But the validity of the theory does not rest on whether deterrence is used to prevent crime or repress legitimate collective action. It rests on whether it works.

The analysis also illustrates that, just because changes in policing predict changes in crime rates, changes in crime rates do not necessarily predict changes in policing. This points once again to the dangers of designating motive or cause on the basis of the effect or function of a structure. That policing affected crime implies neither that the situation was intended nor that crime affected the growth of policing. Whether oriented to consensus or to conflict, arguments that attribute motivation on the basis of known consequences may fit the data no better than do propositions that attribute outcomes on the basis of stated intentions (see also Lieberson [1985] on symmetrical and asymmetrical forms of causation).

Whether the patterns shown by this analysis hold for other states in the 19th century, or for France during earlier or later periods, is uncertain. In some respects, France was unique. For example, the orientation toward surveillance by the French police was more explicit than in the police forces of other countries (Emsley 1983). Also, it is obvious that the expansion of state policing in France could not be responsible for declines in crime during earlier periods when state policing did not exist. However, like protest, surveillance and repression take various forms, evolving together over time, and involving both cultural and structural changes. Different structures will be salient in different periods and in different places (see Bayley 1985). Giddens (1981, 1986), for example, notes that patterns of control in Western countries seem to have evolved from direct repression/deterrence to a more indirect, integrative control through the use of information systems and the industrial economy (see Foucault 1975; also Skocpol and Finegold 1982). In view of this, the effect of policing in France may have lost much of its punch even by the end of World War I. (Ross [1986] notes that the deterrent effect of certainty often decays over time.) Subsequent declines in crime, then, may have resulted from the emergence and growth of different forms of surveillance. This possibility should be examined by further research.

Further longitudinal studies should be advised that the major effect of variables need not be immediate. The deflationary effects of policing on property crime would not have shown up in this study without lagging

American Journal of Sociology

variables. In fact, without lagged variables, the conclusion of this analysis would have been that the principal effect of policing on serious crime was inflationary, not deflationary.

Part of the problem lies with social theorists who neglect to specify the duration of causal processes and fail to alert researchers to the importance of time. This produces propositions that are similar to a musical opus with an unspecified tempo, and the results may or may not turn out as originally intended. However, the issue is usually less theoretical than methodological. The tendency of sociologists to use cross-sectional designs too frequently results in data that fall short of fully addressing the questions they ask. Whether explicit or implicit, every theory that makes causal statements at least indicates sequence. But in spite of this, and the power of longitudinal designs to shed light on sequence, most social scientific research continues to be largely cross-sectional in design. Even in longitudinal research, cross-sectional reasoning often prevails, so the default on lagging is zero, and the opportunity to examine sequence is missed (see, e.g., McDonald 1976). Careful thought would reveal the inadequacy of these analyses for addressing research questions, especially when they are formulated on the macro level, where processes may take longer to produce measurable results. If their goal is empirically supported theory and a body of sociological knowledge, then sociologists should relax their commitment to cross-sectional approaches and accept at least some of the historian's obsession with sequence and with time.

REFERENCES

Abbott, Andrew. 1983. "Sequences of Social Events: Concepts and Methods for the Analysis of Order in Social Processes." *Historical Methods* 15 (4): 129–47.

Alschuler, Albert. 1979. "Plea Bargaining and Its History." *Law and Society Review* 13:211–46.

Archer, Dane, and Rosemary Gartner. 1984. *Violence and Crime in Cross-national Perspective*. New Haven: Yale University Press.

Bailey, W., J. D. Martin, and L. N. Gray. 1972. "On Punishment and Crime (Chiricos and Waldo): Some Methodological Commentary." *Social Problems* 19 (Fall): 282–89.

Banfield, Edward. 1968. *The Unheavenly City*. Boston: Little, Brown.

Bayley, David H. 1975. "The Police and Political Development in Europe." Pp. 328–79 in *The Formation of National States in Europe*, edited by Charles Tilly. Princeton: Princeton University Press.

—. 1985. *Patterns of Policing: A Comparative International Analysis*. New Brunswick, N.J.: Rutgers University Press.

—. 1986. *Social Control and Political Change*. Princeton: Princeton University Press.

Beattie, J. M. 1986. *Crime and the Courts in England, 1660–1830*. Princeton: Princeton University Press.

Beirne, Piers. 1987. "Adolphe Quetelet and the Origins of Positive Criminology." *American Journal of Sociology* 92 (5): 1140–69.

Crime

Black, Donald. 1976. *The Behavior of Law*. New York: Academic.

Boritch, Helen, and John Hagan. 1987. "The Changing Forms of Class and Crime Control: Policing Public Order in 'Toronto the Good': 1859–1955." *Social Forces* 66:307–35.

Bradshaw, Y., and L. Radbill. 1987. "Method and Substance in the Use of Ratio Variables." *American Sociological Review* 52:132–34.

Cameron, Iain A. 1977. "The Police of Eighteenth-Century France." *European Studies Review* 7:47–75.

—. 1981. *Crime and Repression in the Auvergne and the Guyenne, 1720–1790*. Cambridge: Cambridge University Press.

Chambliss, William J. 1964. "A Sociological Analysis of the Law of Vagrancy." *Social Problems* 12:67–77.

Chesnais, Jean-Claude. 1976. *Les Morts violentes en France depuis 1826*. Paris: Presses Universitaires de France.

—. 1981. *Histoire de la violence*. Paris: Lafont.

Chevalier, Louis. 1973. *Laboring Classes and Dangerous Classes in Paris during the First Half of the Nineteenth Century*, translated by Frank Jellinek. Princeton: Princeton University Press.

Chiricos, T., and G. Waldo. 1970. "Punishment and Crime: An Examination of Some Empirical Evidence." *Social Problems* 18 (Fall): 200–217.

Cicourel, Aaron. 1968. *The Social Organization of Juvenile Justice*. New York: Wiley.

Cobban, Alfred. 1961. *A History of Modern France*, vol. 2: 1799–1871. Middlesex: Penguin.

—. 1965. *A History of Modern France*, vol. 3: 1871–1962. Harmondsworth: Penguin.

Cohen, David, and Eric A. Johnson. 1982. "French Criminality: Urban-Rural Differences in the Nineteenth Century." *Journal of Interdisciplinary History* 12 (3): 477–501.

Cohen, Lawrence E., and Marcus Felson. 1979. "Social Change and Crime Rate Trends: A Routine Activity Approach." *American Sociological Review* 44:588–608.

Davidovitch, André. 1961. "Criminalité et répression en France depuis un siècle: 1851–1952." *Review Française de Sociologie* 2:30–49.

Dhrymes, P. J. 1971. *Distributed Lags*. San Francisco: Holden-Day.

Donovan, James. 1981. "Justice Unblind: The Juries and the Criminal Classes in France, 1825–1914." *Journal of Social History* 15 (1): 89–107.

Elias, Norbert. (1939) 1978. *The Civilizing Process: The History of Manners*. New York: Pantheon.

—. (1939) 1982. *The Civilizing Process: Power and Civility*. New York: Pantheon.

—. (1969) 1983. *The Court Society*. New York: Pantheon.

Emsley, Clive. 1983. *Policing and Its Context, 1750–1870*. New York: Macmillan.

Erlanger, Howard S. 1976. "Is There a 'Subculture of Violence' in the South?" *Journal of Criminal Law & Criminology* 66 (4): 483–90.

Ferdinand, Theodore. 1967. "The Criminal Patterns of Boston since 1849." *American Journal of Sociology* 73:84–99.

Firebaugh, G., and J. Gibbs. 1985. "User's Guide to Ratio Variables." *American Sociological Review* 50 (5): 713–22.

—. 1987. "Defensible and Indefensible Commentaries." *American Sociological Review* 52:136–41.

Fischer, Claude S. 1980. "The Spread of Violent Crime from City to Countryside, 1955 to 1975." *Rural Sociology* 45:416–34.

Foucault, Michel. 1973. *Moi, Pierre Rivière, ayant égorgé ma mère, ma soeur et mon frère . . .* Paris: Gallimard.

—. 1975. *Surveiller et punir: naissance de la prison*. Paris: Gallimard.

American Journal of Sociology

France, Ministère de l'Économie nationale. 1872–1913. *Annuaire statistique*. Paris: L'Imprimerie nationale.

France, Ministère de la Justice. 1865–1913. *Compte général de l'administration de la justice criminelle en France*. Paris: L'Imprimerie nationale.

Frégier, H. A. 1840. *Des classes dangereuses de la population dans des grandes villes, et des moyens de les rendre meilleurs*, 2 vols. Paris: Baillière.

Furet, François, and Jacques Ozouf. 1977. *Lire et écrire: l'alphabetisation des Français de Calvin à Jules Ferry*. Paris: Editions de Minuit.

Garofalo, James. 1981. "Time: A Neglected Sociological Dimension." Pp. 93–116 in *Theory in Criminology*, edited by Robert Meier. Beverly Hills: Sage.

Gatrell, V. A. C. 1980. "The Decline of Theft and Violence in Victorian and Edwardian England." Pp. 238–338 in *Crime and the Law: History of Crime and the Law in Western Europe since 1500*, edited by V. A. C. Gatrell, B. Lenman, and G. Parker. London: Europa.

Gibbs, Jack P. 1978. "Deterrence, Penal Policy, and the Sociology of Law." *Research in Law and Sociology* 1:101–14.

Giddens, Anthony. 1981. *A Contemporary Critique of Historical Materialism*, vol. 1. London: Macmillan.

———. 1985. *A Contemporary Critique of Historical Materialism*, vol. 2. Oxford: Polity.

Gillis, A. R. 1987. "Crime, Punishment, and Historical Perspectives." *Sociological Forum* 2 (3): 602–9.

Gillis, A. R., and J. Hagan. 1982. "Density, Delinquency, and Design." *Criminology* 19 (4): 514–29.

Given, James B. 1977. *Society and Homicide in Thirteenth Century England*. Stanford: Stanford University Press.

Gurr, Ted Robert. 1976. *Rogues, Rebels, and Reformers: A Political History of Urban Crime and Conflict*. Beverly Hills: Sage.

———. 1979. "On the History of Violent Crime in Europe and America." Pp. 353–74 in *Violence in America: Historical and Comparative Perspectives*, edited by H. D. Graham and T. R. Gurr. Beverly Hills: Sage.

———. 1981. "Historical Trends in Violent Crime: A Critical Review of the Evidence." *Crime and Justice: An Annual Review of Research* 3:295–353.

Gurr, Ted Robert, Peter N. Grabosky, and Richard C. Hula. 1977. *The Politics of Crime and Conflict: A Comparative History of Four Cities*. Beverly Hills: Sage.

Hagan, J., A. R. Gillis, and J. Chan. 1979. "Explaining Official Delinquency: A Spatial Study of Class, Conflict, and Control." *Sociological Quarterly* 19 (Summer): 386–98.

Haller, Mark. 1970. "Urban Crime and Criminal Justice: The Chicago Case." *Journal of American History* 57:619–35.

Hay, Douglas. 1975. "Property, Authority and the Criminal Law." Pp. 17–64 in *Albion's Fatal Tree: Crime and Society in Eighteenth-Century England*, edited by Douglas Hay et al. New York: Pantheon.

Heumann, Milton. 1975. "A Note on Plea Bargaining and Case Pressure." *Law and Society Review* 9:515–28.

Ignatieff, Michael A. 1978. *A Just Measure of Pain: The Penitentiary in the Industrial Revolution, 1750–1850*. New York: Pantheon.

Jacobs, Jane. 1961. *The Death and Life of Great American Cities*. New York: Random House.

Johnston, J. 1984. *Econometric Methods*, 3d ed. New York: McGraw-Hill.

Kmenta, J. 1971. *Elements of Econometrics*. New York: Macmillan.

Kraft, M. 1987. "On 'User's Guide to Ratio Variables.'" *American Sociological Review* 52:135–36.

Crime

Lane, Roger. 1967. *Policing the City: Boston 1822–1885*. Cambridge: Harvard University Press.

_____. 1974. "Crime and the Industrial Revolution: British and American Views." *Journal of Social History* 7:287–303.

_____. 1980. "Urban Homicide in the Nineteenth Century: Some Lessons for the Twentieth." Pp. 91–109 in *History and Crime: Implications for Criminal Justice Policy*, edited by James A. Inciardi and Charles E. Faupel. Beverly Hills: Sage.

Lebigre, Arlette. 1979. "La naissance de la police en France." *L'histoire* 17:5–12.

LeGoff, T. J. A., and D. M. G. Sutherland. 1974. "The Revolution and the Rural Community in Eighteenth Century Brittany." *Past and Present* 62 (February): 96–119.

Lieberson, Stanley. 1985. *Making It Count: The Improvement of Social Research and Theory*. Berkeley and Los Angeles: University of California Press.

Lodhi, Abdul Q., and Charles Tilly. 1973. "Urbanization, Crime, and Collective Violence in 19th-Century France." *American Journal of Sociology* 79:296–318.

Lyman, J. L. 1964. "The Metropolitan Police Act of 1829." *Journal of Criminal Law, Criminology and Police Science* 55 (1): 141–54.

Magraw, Roger. 1983. *France 1814–1915: The Bourgeois Century*. Oxford: Fontana.

McCleary, R., B. C. Nienstedt, and J. M. Erven. 1982. "Uniform Crime Reports as Organizational Outcomes: Three Time Series Quasi-Experiments." *Social Problems* 29 (4): 361–72.

McDonald, Lynn. 1976. *The Sociology of Law and Order*. London: Faber & Faber.

McQuillan, Kevin. 1984. "Modes of Production and Demographic Patterns in Nineteenth-Century France." *American Journal of Sociology* 89 (6): 1324–46.

Messner, Stephen, and Judith R. Blau. 1987. "Routine Leisure Activities and Rates of Crime: A Macro-Level Analysis." *Social Forces* 65:1035–52.

Milgram, S. 1970. "The Experience of Living in Cities." *Science* 167 (3924): 1461–68.

Miller, W. 1958. "Lower Class Culture as a Generating Milieu of Gang Delinquency." *Journal of Social Issues* 14 (3): 5–19.

Monkkonen, Eric H. 1975. *The Dangerous Class: Crime and Poverty in Columbus, Ohio, 1860–1885*. Cambridge: Harvard University Press.

_____. 1981. *Police in Urban America, 1860–1920*. New York: Cambridge University Press.

Nettler, Gwynn. 1970. *Explanations*. New York: McGraw-Hill.

_____. 1984. *Explaining Crime*, 3d ed. New York: McGraw-Hill.

O'Brien, Patricia. 1978. "Crime and Punishment as Historical Problem." *Journal of Social History* 11 (June): 508–20.

_____. 1982. *The Promise of Punishment: Prisons in Nineteenth-Century France*. Princeton: Princeton University Press.

Ostrom, Charles W., Jr. 1978. *Time Series Analysis: Regression Techniques*. Beverly Hills: Sage.

Perrot, Michelle. 1975. "Délinquance et système pénitentiaire en France au XIX^e siècle." *Annals* 30:67–92.

Polanyi, Karl. 1944. *The Great Transformation*. New York: Praeger.

Quetelet, Adolphe. (1842) 1969. *A Treatise on Man*. A facsimile reproduction by Solomon Diamond. Gainesville, Fla.: Scholars' Facsimiles and Reports.

Ross, H. Laurence. 1986. "Implications of Drinking-and-Driving Law Studies for Deterrence Theory." Pp. 159–69 in *Critique and Explanation: Essays in Honor of Gwynn Nettler*, edited by Timothy F. Hartnagel and Robert A. Silverman. New Brunswick, N.J.: Transaction.

Rudé, George. 1964. *The Crowd in History: A Study of Popular Disturbances in France and England, 1730–1848*. New York: Wiley.

American Journal of Sociology

Sampson, Robert J. 1986. "Effects of Socioeconomic Context on Official Reaction to Juvenile Delinquency." *American Sociological Review* 51:876–85.

Sampson, Robert J., and W. Byron Groves. 1988. "Community Structure and Crime: Testing Social Disorganization Theory." *American Journal of Sociology* 94:774–802.

Silver, Allan. 1967. "The Demand for Order in Civil Society: A Review of Some Themes in the History of Urban Crime, Police, and Riot." Pp. 1–25 in *The Police: Six Sociological Essays*, edited by David Bordua. New York: Wiley.

Simmel, G. 1970. "The Metropolis and Mental Life." Pp. 777–88 in *Neighborhood City and Metropolis*, edited by Robert Guttman and David Popenoe. New York: Random House.

Skocpol, Theda, and Kenneth Finegold. 1982. "State Capacity and Economic Intervention in the Early New Deal." *Political Science Quarterly* 97 (2): 255–78.

Slovak, Jeffery S. 1986. *Styles of Urban Policing: Organization, Environment, and Police Styles in Selected American Cities*. New York: New York University Press.

Smith, Douglas R. 1986. "The Neighborhood Context of Police Behavior." Pp. 313–41 in *Communities and Crime*, edited by A. J. Reiss, Jr., and M. Torry. Chicago: University of Chicago Press.

Stead, Philip John. 1984. *The Police of France*. New York: Macmillan.

Stewart, Jon. 1984. *Understanding Econometrics*. London: Hutchinson.

Stinchcombe, Arthur L. 1963. "Institutions of Privacy in the Determination of Police Administration Practice." *American Journal of Sociology* 69:150–60.

Stone, Lawrence. 1967. *The Crisis of the Aristocracy, 1558–1641*. New York: Oxford University Press.

_____. 1977. *The Family, Sex and Marriage in England, 1500–1800*. Harmondsworth: Penguin.

_____. 1983. "Interpersonal Violence in English Society, 1300–1980." *Past and Present* 101:22–33.

Tilly, Charles. 1985. "Warmaking and Statemaking as Organized Crime." Pp. 141–54 in *Bringing the State Back In*, edited by Peter Evans, D. Rueschemeyer, and T. Skocpol. Cambridge: Cambridge University Press.

_____. 1986. *The Contentious French*. Cambridge: Belknap.

_____. 1987. *How War Made States and Vice Versa*. CSSC Working Paper no. 42, Center for Studies of Social Change, New School of Social Research, New York.

Tilly, C., A. Levett, A. Q. Lodhi, and F. Menger. 1975. *How Policing Affected the Visibility of Crime in Nineteenth-Century Europe and America*. CRSO Working Paper no. 115, Center for Research on Social Organization, University of Michigan, Ann Arbor.

Tittle, C. 1969. "Crime Rates and Legal Sanctions." *Social Problems* 16 (Spring): 409–23.

Tombs, R. 1980. "Crime and the Security of the State: The 'Dangerous Classes' and Insurrection in Nineteenth-Century Paris." Pp. 214–37 in *Crime and the Law: The Social History of Crime in Western Europe since 1500*, edited by V. A. C. Gatrell, Bruce Lenman, and Geoffrey Parker. London: Europa.

Turk, Austin T. 1982. *Political Criminality: The Defiance and Defence of Authority*. Beverly Hills: Sage.

Van de Walle, Etienne. 1974. *The Female Population of France in the Nineteenth Century: A Reconstruction of 82 Départements*. Princeton: Princeton University Press.

Weber, Eugen. 1976. *Peasants into Frenchmen: The Modernization of Rural France, 1870–1914*. Stanford: Stanford University Press.

_____. 1986. *France: Fin de siècle*. Cambridge: Belknap.

Wirth, L. 1938. "Urbanism as a Way of Life." *American Journal of Sociology* 44:3–24.

Crime

Wolfgang, Marvin, and Franco Ferracuti. 1967. *The Subculture of Violence*. London: Tavistock.

Wright, Gordon. 1981. *France in Modern Times*, 3d ed. New York: Norton.

_____. 1983. *Between the Guillotine and Liberty: Two Centuries of the Crime Problem in France*. New York: Oxford University Press.

Zehr, Howard. 1975. "The Modernization of Crime in Germany and France, 1830–1913." *Journal of Social History* 8:117–41.

Zukin, Sharon. 1980. "A Decade of the New Urban Sociology." *Theory and Society* 9:539–74.

The Social Meaning of Money: “Special Monies”¹

Viviana A. Zelizer
Princeton University

Classic interpretations of the development of the modern world portray money as a key instrument in the rationalization of social life. Money is reductively defined as the ultimate objectifier, homogenizing all qualitative distinctions into an abstract quantity. This paper shows the limits of such a purely utilitarian conception of “market money.” A model of “special monies” is proposed to examine the extraeconomic, social basis of modern money. The article argues that, while money does indeed transform items, values, and sentiments into numerical cash equivalents, money itself is shaped in the process. Culture and social structure mark the quality of money by institutionalizing controls, restrictions, and distinctions in the sources, uses, modes of allocation, and even the quantity of money. The changing social meaning and structure of domestic money, specifically married women’s money in the United States, 1870–1930, are examined as an empirical case study of a special money.

In Rossel Island, a small traditional community in the southwestern Pacific, the gender of money was tangibly identified—separate lower-value coins were reserved exclusively for women. And in Yap, one of the Caroline Islands in the west Pacific, mussel shells strung on strings served as women’s money, while men monopolized the more desirable large stones (Baric 1964, pp. 422–23; Sumner [1906] 1940, p. 140). In contrast to the money in these primitive societies, modern money seems starkly

¹ Bernard Barber’s theoretical guidance was indispensable in writing this paper. I am also grateful to Jeffrey C. Alexander, Michael B. Katz, Eviatar Zerubavel, and two *AJS* referees for their helpful suggestions. My research was initially supported by a National Endowment for the Humanities grant, and the paper was completed while I was a visiting scholar at the Russell Sage Foundation as part of its program in behavioral economics. At Russell Sage, Robert K. Merton, Charles Tilly, and Eric Wanner offered valuable comments on an earlier version of this article. Rosann Rovento provided efficient research assistance. A modified version of this article will appear as a chapter in my book *The Social Meaning of Money: “Special Monies”* (Basic Books, forthcoming). Requests for reprints should be sent to Viviana A. Zelizer, Department of Sociology, Princeton University, Princeton, New Jersey 08544.

homogeneous and surely genderless. Yet, camouflaged by the physical anonymity of our dollar bills, modern money is also routinely differentiated, not just by varying quantities but also by its special diverse qualities. We assign different meanings and designate separate uses for particular kinds of monies. For instance, a housewife's pin money or her allowance is treated differently from a wage or a salary, and each surely differs from a child's allowance. Or a lottery winning is marked as a different kind of money from an ordinary paycheck. The money we obtain as compensation for an accident is not quite the same as the royalties from a book. Not all dollars are equal.

But while there is an extensive literature dealing with primitive currency, the sociological bibliography on money remains remarkably sparse. Money is ignored, Randall Collins (1979, p. 190) has suggested, "as if it were not sociological enough."² Significantly, the *International Encyclopedia of the Social Sciences* devotes over 30 pages to money but not one to its social characteristics. There are essays on the economic effect of money, on quantity theory, on velocity of circulation, and on monetary reform but nothing on money as a "*réalité sociale*," using Simiand's apt term (1934). As a result, money remains confined primarily to the economists' intellectual domain; its noneconomic aspects have not been systematically explored.

The dominant utilitarian understanding of money is hardly surprising. It is a by-product of what Bernard Barber (1977) has called the "absolutization of the market": the illusory yet pervasive assumption that market exchange is free from cultural or social constraints.³ And money, as the most material representation of market exchange, seems eminently exempt from extraeconomic influences. To be sure, Veblen ([1899] 1953) alerted us to the social meaning of what money buys, and others have significantly furthered the social, cultural, and historical analysis of consumerism (see, e.g., Parsons and Smelser 1956; Rainwater 1974; Sahlins 1976; Douglas and Isherwood 1979; Horowitz 1985; Schudson 1984; Ap-

² For some exceptions, see Turner (1986), Smelt (1980), and Cheal (1988). To be sure, sociologists have recognized the symbolic and social meanings of money in various empirical settings but only in an ad hoc, nonsystematic way. Goffman (1961) remarked on the absence of any framework for understanding differences and similarities between coercive, economic, and social payments.

³ Granovetter (1985) chides sociologists for implicitly accepting the economists' assumptions that market processes are invulnerable to social influences and therefore unsuitable objects of sociological study. On recent theoretical and empirical advances of the "new economic sociology," see Swedberg (1987). While Granovetter focuses on structural constraints of markets, Sahlins (1976) presents a powerful cultural critique of market determinism. For a provocative, historically grounded alternative to "market culture" and utilitarian conceptions of money, see Reddy (1984, 1987). Brown (1959) offers a psychoanalytical critique of the rational model of money.

American Journal of Sociology

padurai 1986; Miller 1987). But the “freedom” of money itself is not directly challenged.

My article will argue that the utilitarian approach to money is a theoretical and empirical straitjacket. Money belongs to the market, but not exclusively so. And while money is indeed an objective means of rational calculation, it is not only that. I turn first to the traditional interpretation of money, that is, as “market money,” and then propose an alternative model of “special monies” that incorporates the social and symbolic significance of money. In the third part of the article, I present a historical case study of domestic money as one example of a special money. I will argue that domestic money—which includes wife’s money, husband’s money, and children’s money—is a special category of money in the modern world. Its meanings, uses, allocation, and even quantity are partly determined by considerations of economic efficiency, but domestic money is equally shaped by changing cultural conceptions of money and of family life as well as by power relationships, age, and gender. And while in certain respects domestic money transcends social class differences, I will show how class profoundly marks not only its quantity but its quality.

More specifically, my discussion will focus on the changing meaning of married women’s money between the 1870s and 1930s, showing how this money, whether given by the husband or earned in the household or in the labor market, was marked as a different form of currency from an ordinary dollar. It was obtained in special ways, used for designated purposes, and even had a special vocabulary: allowance, pin money, “egg money,” “butter money,” spending money, pocket money, gift, or “dole,” but seldom wage, salary, paycheck, or profit.

MARKET MONEY: A UTILITARIAN APPROACH TO MONEY

To be sure, money occupies a central place in classic interpretations of the development of the modern world. But what kind of place? For Simmel and Weber, money was a key instrument in the rationalization of social life. On purely technical grounds, the possibility of money accounting was essential for the development of rational economic markets. As “the most abstract and ‘impersonal’ element that exists in human life,” as Weber ([1946] 1971, p. 331; [1922] 1978, p. 86) defined it, money became “the most ‘perfect’ means of economic calculation.” It transformed the world, observed Simmel ([1908] 1950, p. 412), into an ‘arithmetic problem.’

Presumably, the fundamental and revolutionary power of money came from its complete indifference to values. Money was perceived as the prototype of an instrumental, calculating approach; in Simmel’s ([1900]

1978, p. 211) words, money was “the purest reification of means.” It was also the symbol of what, in his *Philosophy of Money*, Simmel (1978, p. 280) identified as a major tendency of modern life—the reduction of quality to quantity: “which achieves its highest and uniquely perfect representation in money.” Unlike any other known substance or product, money was the absolute negation of quality. Only money, argued Simmel, “is free from any quality and exclusively determined by quantity.” And therefore, only with money, “we do not ask what and how, but how much” (1978, pp. 279, 259).

That “uncompromising objectivity” allowed money to function as the “technically perfect” medium of modern economic exchange. Free from subjective restrictions, indifferent to “particular interests, origins, or relations,” money’s liquidity and divisibility were infinite, making it “absolutely interchangeable” (1978, pp. 373, 128, 441). The very essence of money, claimed Simmel, was its “unconditional interchangeability, the internal uniformity that makes each piece exchangeable for another.” Money thus served as the fitting neutral intermediary of a rational, impersonal market, “expressing the economic relations between objects . . . in abstract quantitative terms, without itself entering into those relations” (1978, pp. 427, 125).

Noneconomic restrictions in the use of money were unequivocally dismissed by Simmel as residual atavisms: “The inhibiting notion that certain amounts of money may be ‘stained with blood’ or be under a curse are sentimentalities that lose their significance completely with the growing indifference of money” (1978, p. 441). As money became nothing but “mere money,” its freedom was apparently unassailable and its uses unlimited. Thus, for Simmel, money’s “purely negative quality” guaranteed its unbounded flexibility and indiscriminate intrusiveness. With money, all qualitative distinctions between objects were equally convertible into an arithmetically calculable “system of numbers” (1978, p. 444).

This quantification of quality was perceived to be a morally dangerous alchemy. In his early essay “The Power of Money in Bourgeois Society,” Marx ([1844] 1964, p. 169) had warned that the transformational powers of money subverted reality: “confounding and compounding . . . all natural and human qualities . . . [money] serves to exchange every property for every other, even contradictory, property and object: it is the fraternization of impossibilities.” As the “god among commodities” (Marx [1858–59] 1973, p. 221), money emerged as the ultimate objectifier, obliterating all subjective connection between objects and individuals and debasing personal relations into calculative instrumental ties.

Indeed, money fetishism, argued Marx in the *Grundrisse* (1973, p. 222) and in *Capital* ([1867] 1984, p. 96), was the most “glaring” form of commodity fetishism. The “perverted” (Marx [1858] 1972, p. 49) process by

which social relations between individuals were transmuted into material relations between things peaked with money. For other commodities might retain their more “natural” value or “use value” and therefore some distinctive quality. But as pure exchange value, money necessarily assumed an “unmeaning” (Marx 1984, p. 103) form, which in turn neutralized all possible qualitative distinctions between commodities. In their money form, noted Marx (1984, p. 111), “all commodities look alike.” And more incongruously still, money turned even intangible objects devoid of utility—such as conscience or honor—into ordinary commodities. Thus the priceless itself surrendered to price. “Not even the bones of saints . . . are *extra commercium hominum* able to withstand the alchemy” (Marx 1984, pp. 132, 105).

For Marx (1984, p. 132), money was thus an irresistible and “radical leveler,” invading all areas of social life. By homogenizing all qualitative distinctions into an abstract quantity, money allowed the “equation of the incompatible” (Marx 1973, p. 163). Echoing Marx’s vocabulary half a century later, Simmel (1950, p. 414) dubbed money a “frightful leveler,” perverting the uniqueness of personal and social values: “With its colorlessness and indifference . . . [money] hollows out the core of things . . . their specific value, and their incomparability.” And in his essay “Religious Rejections of the World,” Weber (1971, p. 331) noted a fundamental antagonism between a rational money economy and a “religious ethic of brotherliness.”

In an essay published in 1913 in the *American Journal of Sociology*, Cooley submitted a dissenting argument in defense of the dollar. While acknowledging the extension of the cash nexus in modern society, Cooley refused to see money as a necessary antagonist of nonpecuniary values. Instead, sounding much like the 18th-century advocates of what Hirschman (1986) calls the “*doux commerce*” thesis of the market as a moralizing agent, Cooley (1913, p. 202) argued that “the principle that everything has a price should be enlarged rather than restricted . . . pecuniary values are members of the same general system as the moral and aesthetic values, and it is their function to put the latter upon the market.” Progress, concluded Cooley (1913, p. 203), lay not in depreciating monetary valuation but in assuring the moral regulation of money: “The dollar is to be reformed rather than suppressed.” But Cooley’s outlook was exceptional. For most contemporary observers, the dollar was an invulnerable transformer, not a morally reformable currency.

The prevailing classic interpretation of money thus absolutized a model of market money, shaped by the following five underlying assumptions:

1. The functions and characteristics of money are defined strictly in economic terms. Money, maintained Simmel (1978, p. 101), was the “incarnation and purest expression of the concept of economic value.” As a

Money

qualityless, absolutely homogeneous, infinitely divisible, liquid object, money is a matchless tool for market exchange.

2. All monies are the same in modern society. What Simmel called money's "qualitatively communistic character" (1978, p. 440) aborts any distinctions between types of money. Differences can exist in the quantity of money but not in its meaning. Thus, there is only one kind of money—market money.

3. A sharp dichotomy is established between money and nonpecuniary values. Money in modern society is defined as essentially profane and utilitarian in contrast to noninstrumental values. Money is qualitatively neutral; personal, social, and sacred values are qualitatively distinct, unexchangeable, and indivisible.

4. Monetary concerns are seen as constantly enlarging, quantifying, and often corrupting all areas of life. As an abstract medium of exchange, money has not only the freedom but also the power to draw an increasing number of goods and services into the web of the market. Money is thus the vehicle for an inevitable commodification of society. As Simmel put it, money "intervenes in the totality of existential interests and imposes itself upon them . . . [it] has the power to lay down forms and directions for contents to which [it is] indifferent. . ." (1978, p. 442).

5. The power of money to transform nonpecuniary values is unquestioned, while the reciprocal transformation of money by values is seldom conceptualized or else is explicitly rejected. Unfettered by "objective or ethical considerations," money, insisted Simmel, was exempt from extraeconomic "directives [or] obstacles" (1978, p. 441).⁴

In this context, to speak about the distinctive "quality" of modern money seems anachronistic. After all, how can money have special meanings if its very essence is the absolute homogenization and objectivization of qualitative distinctions?

A link, an interdependence, is missing from the traditional approach to money. Impressed by the fungible, impersonal characteristics of money, traditional social thinkers emphasized its instrumental rationality and apparently unlimited capacity to transform products, relationships, and sometimes even emotions into an abstract and objective numerical equivalent. But money is neither culturally neutral nor morally invulnerable. It may well "corrupt" values into numbers, but values and sentiment recip-

⁴ In fact, the only recognized limits to the commodification process is the preservation—albeit precarious—of selected items outside the cash nexus. This "singularization" of certain goods, as Igor Kopytoff (1986) describes it, does not, however, seem to include money. Instead, culture "marks" certain items as special and unexchangeable precisely by depriving them of a price tag (see Radin 1987). Within this framework, money acts as a "contaminator" of market values, immune to extraeconomic values and thus incapable of being itself marked as singular, unique, or unexchangeable.

American Journal of Sociology

rocally corrupt money by investing it with moral, social, and religious meaning. We need to examine more carefully how cultural and social structural factors influence the uses, meaning, and even quantity of money. What is the relationship of money as a medium of exchange and measure of utility to money as a symbol of social value?

SPECIAL MONIES: EXPLORING THE QUALITY OF MODERN MONEY

Significantly, even when the symbolic dimension of modern money has been recognized, the analysis stops short of fully transcending the utilitarian framework. Parsons (1971a, p. 241; 1971b, pp. 26–27), for instance, explicitly and forcefully called for a “sociology of money” that would treat money as one of the various generalized symbolic media of social interchange, along with political power, influence, and value commitments. In contrast to Marx’s (1973, p. 222) definition of money as the “material representative of wealth,” in Parsons’s media theory money was a shared symbolic language; not a commodity, but a signifier, devoid of use value. Yet Parsons restricts the symbolism of money to the economic sphere. Money, Parsons (1967, p. 358) contends, is the “symbolic ‘embodiment’ of economic value, of what economists in a technical sense call ‘utility.’ ” Consequently, the symbolic meaning of money outside the market, money’s cultural and social significance beyond utility, remains uncharted in Parsons’s media theory.

Anthropologists provide some intriguing insights into the extraeconomic, symbolic meaning of money but only with regard to primitive money. For instance, ethnographic studies show that, in certain primitive communities, money attains special qualities and distinct values independent of quantity. How much money is less important than *which* money. Multiple currencies, or “special-purpose” money, to use Karl Polanyi’s (1957, pp. 264–66) term, have sometimes coexisted in one and the same village, each currency having a specified, restricted use (for purchase of only certain goods or services), special modes of allocation and forms of exchange (see, e.g., Bohannan 1959), and, sometimes, designated users. Certain currencies, for instance, may be limited to specified social classes or else assigned by gender (see Einzig 1966).

Special monies are often morally or ritually ranked: certain kinds of money may be good for obtaining food but not for purchasing a wife; other monies are appropriate only for funeral gifts or marriage gifts or as blood money; still other monies serve exclusively for paying damages for adultery or insults, for burial with the dead, or for magical rites. In this context, the “wrong” quality or lesser-quality money, even in large quantities, is useless or degraded. This qualitative categorization of monies

Money

was also noted by Thomas and Znaniecki ([1918–20] 1958, pp. 164–65) in their analysis of the traditional Polish peasant culture: “A sum received from selling a cow is qualitatively different from a sum received as a dowry, and both are different from a sum earned outside.” Different monies were used differently and even kept separately. Indeed, Thomas and Znaniecki (1958, p. 166) remarked that a peasant who set a sum aside for a designated purpose, and then needed some money for a different expense, would prefer to borrow it “even under very difficult conditions, rather than touch that sum.”

These special monies, which Mary Douglas (1967) has perceptively identified as a sort of primitive coupon system, control exchange by rationing and restricting the use and allocation of currency. In the process, money sometimes performs economic functions by serving as a medium of exchange, but it also functions as a social and sacred “marker,” used to acquire or amend status or to celebrate ritual events. The point is that primitive money is transformable, from fungible to nonfungible, from profane to sacred.

But what about modern money? Has modernization indeed stripped money of its cultural meaning, establishing, as Simmel (1978, p. 276) saw it, an “unconditional identity of money with sum”? Economic development, suggested Thomas and Znaniecki (1958, p. 65), “tends to abolish all [the] distinctions and . . . make money more and more fluid.” Influenced by economic models, most interpretations thus establish a sharp dichotomy between primitive, restricted “special purpose” money and modern “all-purpose” money, which, as a single currency unburdened by ritual or social controls, can function effectively as a universal medium of exchange. Curiously, when it comes to modern money, even anthropologists seem to surrender their formidable analytical tools. For instance, more than 20 years ago, Mary Douglas (1967), in an important essay, suggested that modern money may not be as unrestricted and “free” after all. Her evidence, however, is puzzlingly limited. Modern money, argues Douglas (p. 139), is controlled and rationed in two situations: in international exchange and at the purely individual personal levels, where “many of us try to primitivize our money . . . by placing restrictions at the source, by earmarking monetary instruments of certain kinds for certain purposes, by only allowing ourselves or our wives certain limited freedoms in the disposal of money.” “Money from different sources” observes Douglas (p. 139), “is sometimes personalized and attracts distinctive feelings which dictate the character of its spending.”

Surely, these restraints, which, as Douglas (1967, pp. 119–20) notes, “resemble strangely . . . restraints on the use of some primitive monies,” are more than purely individual “quirks” or a “clumsy attempt to control the all too liquid state of money,” as she suggests (pp. 138, 140). Yet

American Journal of Sociology

Douglas, who (Douglas and Isherwood 1979) significantly advances a cultural theory of consumption, does not go far enough with the cultural analysis of money. Likewise, Thomas Crump (1981, pp. 125–30) refers to the existence of what he calls “bounded sub-systems” in modern societies: separate spheres of exchange with special currencies. But his focus is on economic distinctions between types of monies, such as the simultaneous yet separate use of a national and a foreign currency (usually the dollar) by a country, the selective use of specie versus “scriptural” money for certain goods and services, or the separate economy of credit cards versus cash payments, and includes even the chips used by a poker school as a separate form of currency (see also Melitz 1970).

Economic psychologists have recently challenged the purely rationalistic economic definition of modern money, particularly the idea of fungibility, by suggesting the concept of “mental accounting”: the ways individuals distinguish between kinds of money. For instance, they treat a windfall income much differently from a bonus or an inheritance, even when the sums involved are identical (see, e.g., Thaler 1985; Kahneman and Tversky 1982; for an excellent review and analysis of the psychological literature on money, see Lea, Tarpy, and Webley 1987, pp. 319–42).

But mental accounting cannot be fully understood without a model of “sociological accounting.” Modern money is marked by more than individual whim or the different material form of currencies. As François Simiand, one of Durkheim’s students, argued (1934), the extraeconomic, social basis of money remains as powerful in modern economic systems as it was in primitive and ancient societies.⁵ Indeed, Simiand warned against an orthodox rationalist approach that mistakenly ignores the persistent symbolic, sacred, and even magical significance of modern money.

My general theoretical purpose, then, is to apply the concept of special money to the modern world and examine in what ways culture and social structure mark modern money by introducing controls, restrictions, and distinctions that are as influential as the rationing of primitive money. Special money in the modern world may not be as easily or visibly identifiable as the shells, coins, brass rods, or stones of primitive communities, but its invisible boundaries emerge from sets of formal and

⁵ Sorokin (1943) made the same argument in his brilliant analysis of the persistence of qualitative distinctions in modern conceptions of time and space. Taussig (1980), an anthropologist, deals with the social meaning of modern money but within the particular context of a South American peasant culture’s being transformed by capitalist modes of production. The peasants construct magical rituals that mark modern money as “dirty money”: an evil currency obtained in illicit ways and restricted in its uses. Taussig interprets these morally stigmatized monies as part of the peasants’ resistance to the commodification of their world. From this perspective, however, real modern money in a fully commoditized society would presumably lose such qualitative distinctions and thus attain moral indifference.

Money

informal rules that regulate its uses, allocation, sources, and quantity. How else, for instance, do we distinguish a bribe from a tribute or a donation, a wage from an honorarium, or an allowance from a salary? How do we identify ransom, bonuses, tips, damages, or premiums? True, there are quantitative differences among these various payments. But, surely, the special vocabulary conveys much more than diverse amounts. Detached from its qualitative differences, the world of money becomes undecipherable.

The model of special monies thus challenges the traditional utilitarian model of market money by introducing different fundamental assumptions in the understanding of money:

1. While money does serve as a key rational tool of the modern economic market, it also exists outside the sphere of the market and is profoundly shaped by cultural and social structural factors.

2. There are a plurality of different kinds of monies; each special money is shaped by a particular set of cultural and social factors and is thus qualitatively distinct. Market money does not escape extraeconomic influences but is in fact one type of special money, subject to particular social and cultural influences.

3. The classic economic inventory of money's functions and attributes, based on the assumption of a single general-purpose type of money, is thus unsuitably narrow. By focusing exclusively on money as a market phenomenon, it fails to capture the very complex range of characteristics of money as a nonmarket medium. A different, more inclusive coding is necessary, for certain monies can be indivisible (or divisible but not in mathematically predictable portions), nonfungible, nonportable, deeply subjective, and therefore qualitatively heterogeneous.

4. The assumed dichotomy between a utilitarian money and nonpecuniary values is false, for money under certain circumstances may be as singular and unexchangeable as the most personal or unique object.

5. Given the assumptions above, the alleged freedom and unchecked power of money become untenable assumptions. Culture and social structure set inevitable limits to the monetization process by introducing profound controls and restrictions on the flow and liquidity of money. Extraeconomic factors systematically constrain and shape (a) the *uses* of money, earmarking, for instance, certain monies for specified uses; (b) the *users* of money, designating different people to handle specified monies; (c) the *allocation* system of each particular money; (d) the *control* of different monies; and (e) the *sources* of money, linking different sources to specified uses.

Even the quantity of money is regulated by more than rational market calculation. For instance, in *The Philosophy of Money*, Simmel (1978, pp. 273, 406) suggests that money in "extraordinarily great quantities" can

American Journal of Sociology

circumvent its "empty quantitative" nature: it becomes "imbued with that 'super-additum,' with fantastic possibilities that transcend the definiteness of numbers." The apparent objectivity of numbers, however, is escaped not only by large fortunes. Small sums of money can attain similar distinction. For example, in civil law countries that permit monetary compensation for the grief of losing a child in an accident, legal scholars advocate the "*franc symbolique*" (Mazeaud, Mazeaud, and Tunc 1957). A token sum of money is perceived as the only dignified equivalent for such a purely emotional loss. Thus, determining a proper amount often involves not only an instrumental calculus but a cultural or social accounting. (For an example of the connection between quantity of money and its social and symbolic meaning, see Geertz [1973], pp. 425-42.)

Even identical quantities of money do not "add" up in the same way. A \$1,000 paycheck is not the same money as \$1,000 stolen from a bank or \$1,000 borrowed from a friend. And certain monies remain indivisible—an inheritance, for instance, or a wedding gift of money intended for the purchase of a particular kind of object. The latter is a qualitative unit that should not be spent partly for a gift and partly for groceries.

Exploring the quality of special monies does not deny money's quantifiable and instrumental characteristics but moves beyond them, suggesting very different theoretical and empirical questions from those derived from a purely economic model of market money. Domestic money raises some of those questions. What kind of money circulates within the family? How is it allocated, and how is it used? How do changing social and power relationships between family members affect the meaning of the domestic dollar?

In terms of data, studying money in the family is entering largely uncharted territory. Although money is the major source of husband-wife disagreements and often a sore point between parents and children, curiously, we know less about money matters than about family violence or even marital sex.⁶ Not only are families reluctant to disclose their private financial lives to strangers; husbands, wives, and children often lie, de-

⁶ There is a body of literature that deals with the relationship between gender, class, money and the distribution of family power (see, e.g., Blood and Wolfe 1965; Komarovsky 1961, 1967; Safilios-Rothschild 1970; Rubin 1976; Ostrander 1984; Blumstein and Schwartz 1985; Hertz 1986; Mirowsky 1985). Interestingly, much of this literature retains an instrumental framework by usually focusing on the market meaning of money and its effects on domestic power relationships. Contemporaneous and historical studies of English households provide a rich source of data on intrafamily accounting systems (see, e.g., Ross 1982; Oren 1973; Stearns 1972; Wilson 1987; Pahl 1980; Whitehead 1984; Ayers and Lambertz 1986). (For France, see Sullerot 1966; for French and English working-class households, see Tilly and Scott 1978.) Gullestad (1984) provides some wonderful data on working-class mothers in urban Norway, and Luxton (1980) does the same for Canada.

Money

ceive, or simply conceal information from each other as well. Perhaps more fundamentally still, the model of what Sen (1983) calls the "glued-together family" has meant that questions about how money is divided between family members are seldom even asked. Once money enters the family, it is assumed to be somehow equitably distributed among family members, serving to maximize their collective welfare. How much money each person gets, how he or she obtains it, from whom and for what, are rarely considered. And yet, as Michael Young (1952) suggested more than 30 years ago, the distribution of money among family members is often as lopsided and arbitrary as the distribution of national income among families. Therefore, argues Young (1952, p. 305), we should stop assuming that "some members of a family cannot be rich while others are poor" (see also Hartmann 1981; Wong 1984; Delphy and Leonard 1986).

The period between 1870 and 1930 provides some unusual glimpses into this traditionally secret world of family money. As the consumer society was being established, Americans wrote about and studied money matters in an unprecedented manner. Household-budget studies richly documented how the working class and lower-middle class spent their money. And in anonymous, "confessional" articles published in popular magazines, middle-class Americans disclosed their own domestic budgets, transforming the spending of money into a public issue. In that same period, as Daniel Horowitz (1985) has shown, social critics and social scientists presented their versions of the "morality of spending," discussing with passion and in detail the usually dreaded noneconomic contours of a commoditized American society. Thus, at the turn of the century, the renegotiation of the domestic economy broke through the usually closed doors of individual households and entered the public discourse.

I now turn to an analysis of the changing meanings, allocation systems, and uses of married women's money between 1870 and the 1930s, showing how definitional disputes over this category of domestic money, while partly a rational response to a new economic environment, were also deeply shaped by extraeconomic, social, and cultural factors. The battle over the purse strings was regulated by notions of family life and by the gender and social class of its participants.⁷

⁷ This article is based on a qualitative analysis of an extensive and diversified set of documentary sources. Among the primary sources consulted were (1) household-budget studies; (2) women's magazines, including feature articles, letters to the editor, fiction, advice columns, and occasional survey data; (3) newspapers, including news articles, editorials, and letters to the editor (mostly from the *New York Times*); (4) legal records, including court cases, law review articles, laws and regulations, and legal casebooks; (5) home-economics literature, including leading textbooks on home management, popular household manuals, and the *Journal of Home Economics*; (6) a foreigner's memoirs; (7) etiquette manuals; (8) social workers' investigations of working-class communities and reports on the conditions of working women, such as Rus-

American Journal of Sociology

The Domestic “Fiscal Problem”: 1870–1930

During the late 19th century, the domestic “fiscal problem” went public, as an appealing news story in magazines and newspapers, in poignant letters to the editor and advice columns, and as the topic of conferences in women’s clubs. By 1928, one observer concluded that “More quarrels between husband and wife have been started by the mention of money than by chorus girls, blond waitresses, dancing men with sleek hair, [or] traveling men” (Kelland 1928, p. 12). Indeed, the battle over the purse strings often ended in court. Between 1880 and 1920, money quarrels increasingly became a grounds for divorce among affluent as well as poor couples (May 1980, p. 137; Lynd and Lynd 1956, p. 125). And domestic money raised legal issues even in unbroken marriages. Did a wife have a right to an allowance? If she saved money from her housekeeping expenses, was that money hers? Was a wife a thief if she “stole” money from her husband’s trousers? Could a wife pledge her husband’s credit at any store? There was also the matter of women’s earnings. When was a woman’s dollar legally her own? Slowly, but steadily, court decisions began to overturn the common law dictum that a wife’s earnings belonged to her husband.

Why did domestic money become such a controversial currency at the turn of the century? Certainly money conflicts between family members had existed earlier. For instance, in her study of New York working-class women, Stansell (1986, p. 29) tells of one—albeit extreme—1811 case in which a husband beat his common law wife to death after she took four shillings from his pockets. Yet these disputes remained private, rarely entering the public discourse as a major issue of collective concern (Stansell, personal communication). A consensus of sorts existed about the proper regulation of family income, and it varied by class. Among middle- and upper-class households, money matters seem to have been estab-

sell Sage’s West Side Studies; and (9) selected government documents such as the U.S. Department of Labor reports on the conditions of working women and children. To be sure, as with many such qualitative historical data, the precise representativeness of the documentary materials cannot be established. However, the reliability of the data is strengthened by corroborating the findings with very different and independent types of documentary evidence. For instance, a few magazines’ surveys of readers or other audiences are used, despite their being methodologically weak by modern standards, but only as an additional, albeit imperfect, illustration of trends in the allocation and uses of family money. Thus, the changes in the domestic economy described here are not based simply on one set of data but are amply confirmed by the different primary sources. In addition, a rich set of secondary sources has been consulted, including recent studies on the rise of the consumer culture, the history of leisure, and the literature on family history and women’s history that includes discussions of families’ changing economic strategies.

Money

lished largely as the husband's business. In her (1841) landmark *Treatise on Domestic Economy*, Catherine Beecher noted how, particularly among businessmen, a family's expenses were "so much more under the control of the man than of the woman" (p. 176). Likewise, Mary P. Ryan's (1984, p. 33) study of family life in early 19th-century Oneida County, New York, found men in charge of money matters (see also Norton 1979, p. 145; Cowan 1983, pp. 81–82). After all, the 19th-century "cult of domesticity" established home life as an alternative to the dominance of the market: its guardian, the "true" Victorian woman, was a specialist in affect, not finances (Welter 1966; Cott 1977). A woman might handle the housekeeping expenses, but "serious money" was a man's currency. Working-class households, on the other hand, managed their limited and often uncertain incomes by appointing wives the family's cashier. Husbands and children handed their paychecks over to the wives, who were expected to administer the collective income skillfully. Most of these monies, to be sure, were limited to housekeeping expenses.

But at the turn of the century, a rise in real income and the increasing monetization of the American economy forced a reevaluation of family finances. Making more money and spending it required not only skillful bookkeeping; they also raised a new set of confusing and often controversial noneconomic quandaries. How should money be allocated in the family? How much money should a wife receive and for which expenses? Was an allowance a "good" mechanism of allocation for wives? What about children's allowances; should they be given one, or was it their duty to earn it through household chores? Should husbands hand over all their salaries to their wives, or how much could they keep for themselves?

Proper uses of money also baffled novice consumers: What did it mean to spend money well? How, for instance, should a family's extra income be used? How much should be saved, how much given to charity, how much for vacations, how much for clothing? And, most important, how was it to be determined how much each member of the family was entitled to spend? As the amount of disposable income increased and as the consumer economy and culture became more firmly established, family money was increasingly differentiated into husband's money, wife's money, and children's money.

This new "tightened competition for the family income," as Robert Lynd (1932, p. 90) described it, was to a certain extent a "fixed" dispute, for the various competitors started with culturally assigned "handicaps." Indeed, turn-of-the-century wives, even those married to wealthy men, often found themselves without a dollar of their own. As Lucy M. Salmon, professor of history at Vassar College, explained in 1909, "Men are still for the most part those whose wages are paid in hard cash, who

American Journal of Sociology

have a bank account and carry a cash-book, and who therefore consider that they have the right to decide in regard to the way the money they earn shall be spent" (p. 889).

The relative poverty of married women became increasingly untenable. For the 20th-century version of the 19th-century moral guardian was expected to serve as the household's purchasing agent and budget expert. To be sure, the frugality and financial wisdom of wives had been a concern in the 18th century as well (Beecher 1841, pp. 175–86; Jensen 1986, pp. 119–28). But the expansion of the consumer economy made proper spending skills a dominant and visible parameter of domestic expertise. The "good housekeeper" was responsible "for the care of her husband's money, and she must expend it wisely" (*New York Times*, Dec. 23, 1900, p. 10). After all, as one exemplary housewife explained in the same article, "a man does not understand the regulation of the household and its expenses."

But "Mrs. Consumer's" (Frederick 1929) increased financial role and responsibility came without a salary and most often without even a fixed and dependable income. Women were thus caught in the strange predicament of being cashless money managers expected to spend properly but denied control over money. The success of the home-economics movement, which urged women to run their homes like a business, further intensified the contradiction in women's economic lives.

Women's strategems to extract some cash from their unforthcoming husbands were the subject of jokes and a staple of late 19th-century vaudeville routines. But the domestic fiscal problem turned serious, forcing a difficult and controversial reevaluation of women's household money as well as of their earned income.

A Dollar of Her Own: Defining Women's Household Money

American women, even those whose husbands could afford it, never had a legal claim to any portion of domestic money. As long as spouses lived together, the author of a 1935 *Law Review* article explained, "the wife's right to support is not a right to any definite thing or to any definite amount. . . . Whether the wife will get much or little is not a matter of her legal right but is a matter for the husband to decide" (Crozier 1935, p. 33).⁸ As a result, the allocation of domestic money depended on unofficial

⁸ The concept of a family wage—a salary that would support a male wage earner and his dependent family—further increased married women's dependence on their husband's wages. The doctrine of necessaries, however, provided wives with some legal recourse by making a husband directly responsible to a merchant for the purchases made by his wife. Yet even this entitlement to pledge a husband's credit was restricted.

Money

rules and informal negotiation. At the turn of the century, married women—the majority of whom depended on their husbands' paychecks or incomes—obtained their cash in a variety of forms or special monies.

Upper- and middle-class wives received an irregular dole or, more rarely, a regular allowance from their husbands for housekeeping expenses, including household goods and clothing. Sometimes women relied almost entirely on "invisible" dollars, crediting their expenses and rarely handling cash at all. Working-class wives, on the other hand, were given their husbands' paychecks and were expected to administer and distribute the family money.

These official monies, however, were supervised and even, in the case of working-class women, ultimately owned and controlled by the husbands. Sometimes, husbands openly took over all monetary transactions. In a letter to the advice column of *Woman's Home Companion* in 1905, a 30-year old woman complained that John, her husband, although "liberal in a way . . . keeps the pocketbook himself, buys the provisions, prefers to purchase the dry-goods, the shoes, the gloves . . . and does not see that I need any money when he gets whatever I want" (Sangster 1905, p. 32).

Even if a woman managed to save some money from her housekeeping expenses, the law ultimately considered that money as her husband's property. For instance, in 1914, when Charles Montgomery sued his wife, Emma, for the \$618.12 she had saved from the household expenses during their 25 years of marriage, Justice Blackman of the Supreme Court, Brooklyn, ruled for the husband, arguing "that no matter how careful and prudent has been the wife, if the money . . . belonged to the husband it is still his property, unless the evidence shows that it was a gift to his wife" (*New York Times*, Dec. 16, p. 22). Thus, a wife's channels to additional cash were limited to a variety of persuasion techniques: asking, cajoling, downright begging, or even practicing sexual blackmail.

If these techniques failed, there was also a repertoire of underground financial strategies, ranging from home pocket picking to padding bills. In 1890, an article in the *Forum* denounced the "amount of deceit, fraud, and double dealing which grow out of the administration of the family

Necessaries were so ambiguously defined that merchants were reluctant to risk extending credit to a wife for goods that might not be considered necessities. Moreover, a husband was entitled to determine where necessities should be purchased and could terminate a wife's authority to pledge his credit by demonstrating that he had provided the necessities or a sufficient allowance to obtain them (see Weitzman 1981; Salmon 1986; Clark 1968). The law, in fact, was explicitly concerned with protecting husbands from the "mad" expenditures of extravagant wives (see, e.g., *Ryan v. Wanamaker* 116 Misc. 91; 190 N.Y.S. 250 (1921); *Saks et al. v. Huddleston* 36 F. (2d) 537 (1929); and *W.A.S. 1922*).

American Journal of Sociology

finances." Just to obtain "a few dollars they can call their own," women routinely engaged in systematic domestic fraud: some "get their milliners to send in a bill for forty dollars, instead of thirty, the real price, in order to take the extra ten to themselves . . . [others] overtax their tired eyes and exhausted bodies by taking in sewing without their husband's knowledge; and . . . farmers' wives . . . smuggle apples and eggs into town" (Ives 1890, pp. 106, 111).

Other methods were even riskier. In 1905 Joseph Schultz was taken to the police court of Buffalo by Mrs. Schultz. It seems that Mr. Schultz, determined to stop his wife's nocturnal thefts of the change left in his trousers, set a small rattrap in the trouser pocket. About 2:00 A.M. the trap was sprung, and next morning the husband was taken to court. *Bench and Bar*, a New York legal journal, reported with some satisfaction that the judge turned down the wife's complaint and upheld the right of husbands to maintain rattraps for the protection of their small change (3 *Bench and Bar* 6). In another case, Theresa Marabella, 40 years old, was sentenced to four months in a county jail for stealing \$10 from the trousers of Frank Marabella, a laborer and her husband. She had spent the money on a trip to New York (*New York Times*, July 14, 1921).

But "stolen" dollars were not taken only by the wives of poor men. Indeed, one observer was persuaded that "the money skeletons in the closets of some nominally rich women may be as gruesome as are those in the closets of the nominally poor" (Salmon 1909, p. 389). While poor women rifled their husbands' trousers looking for some change, the affluent cashless wife used a variety of fraudulent techniques. Mrs. Gray, a grandmother married for 20 years but without any money "she could call her own," "adopted a systematic policy of deceit and fraud toward her husband. . . . When she wants to give a little money to help buy a stove for a poor family, or to assist some sick or starv[ing] creature to pay his rent, she tells her husband that the flour is out, or that the sugar is low, and so gets the needful amount." Thus, paradoxically, this "strict church member," who never told a falsehood, "cheats and deceives" the man "she has solemnly sworn to love and obey" (Ives 1890, p. 110).

There were other ways to "circumvent the holder of the purse" (Salmon 1909, p. 889). Women bargained with dressmakers, milliners, and shopkeepers to add extra items to their bills so that, when the bill was paid, "the rich man's wife may get a rake-off and possess a few dollars" (Peattie 1911, p. 466). In search of cash, some women even turned to their servants, selling them their old furniture (O'Hagan 1909). A Japanese visitor to the United States in the 1910s was shocked to hear from "men and women of all classes, from newspapers, novels, lecturers, and once even from the pulpit . . . allusions to amusing stories of women secreting

Money

money in odd places, coaxing it from their husbands, . . . or saving it secretly for some private purpose" (Sugimoto [1926] 1936, p. 176).⁹

As the consumer economy multiplied the number and attractiveness of goods—many of them targeted at a female audience—the demand intensified for a more definite and regular housekeeping income for the wife and increasingly for her “private purse,” a free sum of unaccounted money to spend for the home, for entertainment, or on clothes, cosmetics, perfumes, or gifts. (On the sales strategies of department stores, 1890–1940, aimed at an almost entirely female middle-class clientele, see Benson [1986]; on the commercialization of the beauty industry in the early 20th century, see Banner [1983], pp. 202–25.)

Dole versus Allowance: The Allowance as a Solution

The traditional doling-out method of supplying women with money came under attack by the late 19th century in a battle that continued during the first three decades of the 20th century. Anonymous letters to the editors of women’s magazines conveyed the money troubles of housewives. “What Should Margaret Do?” asked one woman whose husband in 1909 gave her only \$50 a month (from his \$300 salary) to run the house, pay all bills, and clothe herself and a baby girl. When she asked for more, “John . . . gets very angry and accuses her of being dissatisfied . . . [and tells her] she is always wanting something” (*Good Housekeeping* 1909, p. 50).

Condemning a system that forced women to play the “mendicant before a husband,” the well-known and widely syndicated columnist Dorothy Dix (1914, pp. 408–9) remarked on the irony of a man who “will trust [a] wife with his honor, his health, his name, his children, but he will not trust her with money.” The availability of credit was no solution, since it was simply another form of gift money supervised by the husband. Indeed, observers noted the “anomalous” situation in which men willingly paid “large bills . . . [of] wives and daughters” yet were unwilling “to trust them with the smallest amount of ready money” (Salmon 1909, p. 889). The rich wife, remarked the widely read writer and theologian Hugh Black (1921, p. 58), could order “anything from countless stores where they had a charge account. . . .” But often, “she could not give ten cents to a beggar.”

A better system was needed to assure women, as one commentator put it, “the divine right . . . to the pay envelope” (“Family Pocketbook” 1910,

⁹ In her best-selling autobiography, Sugimoto (1936) recalled her puzzlement at this strange American custom that departed so radically from the Japanese arrangement, where, regardless of class, wives controlled the purse strings. I thank Sarane Bocock for this reference.

American Journal of Sociology

p. 15). Even the courts occasionally agreed, refusing to treat domestic stealing as real theft. In a 1908 case of a wife charged with robbing her husband of small change, Judge Furlong of a Brooklyn court supported the "thief," declaring that "a wife has a perfect right to go through her husband's pockets at night and take his money if he fails to provide for her properly" (15 *Bench and Bar* 10).

But what was a proper money income for wives? For some, the best solution for "penniless wives" was a dowry for every daughter (Messinger et al. 1890). Wives seemed to prefer a regular weekly or monthly allowance. A 1910 *Good Housekeeping* survey of 300 wives found that 120 supported the allowance system ("Family Pocketbook"). By 1915, according to *Harper's Weekly*, some young brides, "of the ultra-modern type," required the promise of an allowance "before vowing to love, honor and obey" ("Adventures in Economic Independence," p. 609).

Women's magazines increasingly endorsed the allowance in their articles and even in their fiction. In "Her Weight in Gold," for instance, a short story that appeared in the *Saturday Evening Post* in 1926, Mrs. Jondough, the wealthy female protagonist, declared "that all the gowns and diamond pins in the world were not compensation for even a tiny personal allowance of her very own" (Child 1926, p. 125). That same year, the Women's Freedom League of St. Louis went further, sponsoring a bill that would make a dress allowance for wives legally compulsory (*New York Times*, Oct. 11). Home-economics experts were in agreement. Mary W. Abel (1921, p. 69), an editor of the *Journal of Home Economics*, assailed the dole system, arguing that "to achieve the best results in the spending of the family money, the mother should have such control of the income as will ensure her efficiency as manager and buyer." Even Emily Post certified the allowance with her stamp of approval (Post 1928, p. 110).

Still, converting female currency from dole to allowance was not easily achieved. A 1928 survey of 200 upper-class men and women found that, while 73 used the allowance system, 66 still relied on the "old-fashioned system of husbands taking charge of all money, paying all bills and doling out funds to the wife as she asks for them" (Hamilton and MacGowan 1928) (the remainder had a more progressive joint bank account or an undefined arrangement).¹⁰ Husbands, it seems, were less enthusiastic than their wives about the allowance. As Dix (1914, p. 409) pointed out: "One question that is fought out in a battle that lasts from the altar to the grave, in most families, is the question of an allowance for the wife. She

¹⁰ As described in *Harper's Monthly Magazine*, this survey of financial arrangements was part of a larger study of different aspects of married life conducted under the auspices of the Bureau of Social Hygiene. Using a prepared set of questions, the survey takers interviewed 200 respondents individually over a period of two years.

yearns for it. The man is determined that she shall not have it. . . ." As late as 1938, when the *Ladies Home Journal* conducted a major national survey on "What Do the Women of America Think about Money?" and asked, "Should a wife have a regular housekeeping allowance?" they found that 88% of female respondents answered affirmatively, regardless of marital status or geographical location. And 91% of younger women (under 30) were for it. Yet only 48% of the wives actually received an allowance (Pringle 1938, p. 102).

Husbands resisted the allowance because it officially carved out a separate portion of their income and made it "hers," thereby increasing a woman's financial control. But the allowance created an additional sort of confusion. What kind of money was it? If the allowance was no longer supposed to be a dole or gift, neither could it become real money or a wage. Indeed, supporters of the allowance were careful to distinguish it from a wage. "To the man who says, 'But I cannot pay my wife like a servant,'" recommended a writer in the *Forum*, "the answer must be 'Certainly not. She is a partner and as such is entitled to a share in the dividends'" (Ives 1890, p. 113).

If the allowance was difficult to define, it was also hard to regulate. Since it was not a payment, the actual amount of money involved could not depend on the performance of wifely duties. While usually it was expected to be "proportioned to the earnings of her husband," in practice, as a *New York Times* editorial pointed out, it remained a "delicate question," often creating a "sharp difference of opinion about [its] size" (*New York Times*, Jan. 30, 1923). The uses of the allowance remained unclear as well. Was it exclusively for the household? Who "owned" the surplus, if there was one? Did it cover women's personal needs?

The Allowance versus a Joint Account: The Allowance as a "Bad" Money

In February 1925, Reverend Howard Melish, rector of the Holy Trinity Church of Brooklyn, addressing the New York Women's City Club on the importance of a wife's economic independence, related an anecdote that backfired. "Yesterday," Melish told his audience, "I asked an old lady . . . what her idea was of a happy marriage. Without an instant's hesitation she replied 'An allowance.' " The next day, in an editorial entitled "They Want More than That," the *New York Times* expressed the new critical view on allowances: "Admitting . . . the equality of service rendered by wife and husband in . . . the family unit, why should the one rather than the other have an 'allowance' and . . . why should the 'allowance' be determined by the husband and be granted as a favor?" Allowances, concluded the editorial, "are for inferiors from superiors" and

American Journal of Sociology

therefore an inappropriate currency for the modern woman (*New York Times*, March 2, March 3).

In the 1920s, even as popular support for allowances intensified, there was also a growing criticism of the allowance system from those who saw it as an inequitable and even degrading form of domestic money. Christine Frederick (1919, p. 269) proclaimed it a "relic of some past time when women were supposed to be too inexperienced to handle money." Frederick, a leader of the popular household-efficiency movement, rejected the allowance as an "unbusinesslike" scheme that undermined the modern goal of running the home as rationally as a factory or an office. The "anti-allowance" advocates supported a democratic "joint control of the purse" (Kyrk 1933, pp. 182–83). The new improved domestic money was to be shared, thus minimizing gender as well as age inequality. Families were urged to "hold a periodic council around a table, with frank and courteous discussion of its ways and means, and with due consideration of how, and how much, each member can contribute in work, in money, in cooperation, toward . . . this whole business of the home." The father and mother would act as a board of directors, allocating money according to the diverse needs (Winter 1925, p. 185; Friend 1930, p. 112). The new financial system would also include a specified sum for personal expenses for each family member, to be considered as a budgetary entitlement and not as a gift.

But how many couples actually adopted the new domestic dollar? *Harper's* 1928 study "Marriage and Money" found that, of 200 respondents, only 54 had what the magazine described as the more "feminist" financial arrangement: a joint bank account or common purse (Hamilton and MacGowan 1928, p. 440). In 1929, in *Middletown*, the Lynds (1956, p. 127, n. 24) reported that most couples depended on "all manner of provisional, more or less bickering" financial arrangements. And some two decades later, *Crestwood Heights*, studying suburban life, discovered that, despite democratic norms dictating cooperative spending of the husband's income, "the wife does not know, even roughly, how much her husband earns." Wives still had to "manipulate their household allowances" to obtain "unreported" personal funds (Seeley, Sim, and Loosley 1956, pp. 184–85). Yet, even though the actual finances of housewives did not significantly improve, it is clear that, by 1930, the symbolic meaning of a wife's allowance was changing from a sign of independence and domestic control to a form of financial submissiveness.

A Husband's Allowance: Domestic Money in the Working Class

Domestic money was not defined only by gender but also by the social class of the household. The working-class wife, suggested one home-

Money

economics textbook, could well be envied by wealthier women. While the latter seldom have "ready money in hand," the wife of a workingman often "determines the . . . financial policy of the family and has control of the necessary funds" (Abel 1921, p. 5). Indeed, in her 1917 study, Mary K. Simkhovitch found that as a family's income increased "the proportion controlled by the wife diminishes till often she becomes simply a beneficiary of the husband." Paradoxically, class—in most ethnic groups—seemed to reverse the gender power structure of domestic money. In her 1910 study of Homestead, Margaret F. Byington (1910, p. 108) discovered that the men "are inclined to trust all financial matters to their wives." On payday, workmen turned over their wages to the wife, asking "no questions as to what it goes for."¹¹

In working-class families, the allowance was usually for husbands and children, not wives. Louise B. More's (1907) analysis of wage earner's budgets found that an allowance for "spending-money" was made in 108 of the 200 families she investigated: 94 men received all or part of the amount given, and in 29 families one or two children had an allowance. In most cases, it seems to have been the wife who "doles out spending money according to the needs and the earnings of each" (True 1914, p. 48). Leslie Tentler's (1982, p. 177) study of working-class women from 1900 to 1930 concludes that this financial arrangement of working-class families granted a great deal of economic power to wives, making the home their "fief." Indeed, to contemporary middle-class observers, it appeared that husbands "who accept a daily dole from their purse-keeping wives are usually subject beings" (*New York Times*, Jan. 30, 1923).

But these studies and observations may have idealized and thus overestimated the economic clout of working-class wives. To be sure, administering the family income involved women actively in domestic finances, allowing them a degree of managerial control. What remains unclear, however, is their actual discretionary power.¹² In the first place, money management in families with limited money incomes was an arduous task. Although working-class standards of living improved at the turn of

¹¹ The available evidence suggests that different ethnic groups operated with similar domestic financial arrangements (see, e.g., True 1914; di Leonardo 1984; Bodnar 1987; research material from Morawska 1985, and personal communication). Lamphere (1986), however, suggests possible ethnic variations. Further research should better illuminate the effect of ethnicity on domestic money.

¹² Determining the effect of a particular intrahousehold financial arrangement on the relative power of family members is a difficult task. Not only can power be measured in a number of ways, but all the dimensions of monetary power within the family—whether consuming, saving, investing, or managing—have very special meanings that are culturally and socially constructed. More research is needed to define and understand the relative degree of power of the "cashier" working-class wife.

the century, family budget studies show the precariousness and uncertainty of their financial lives. Husbands' and children's wages went almost exclusively for food, clothing, shelter, and insurance. And being the cashier put a heavy burden of responsibility on wives: household money troubles could be conveniently blamed (by family members as well as outsiders) on female mismanagement rather than on a tight budget or an irregular labor market (Horowitz 1985, p. 60).

More important, as soon as there was any surplus income, a wife's apparent grip on the purse strings quickly loosened. While the ideal good husband was indeed expected to turn over all his wages intact to his wife, receiving one or two dollars a week for his personal use, many did not. Studies of New York's West Side conducted in 1914 found that while "there is a current belief that the American workingman turns his wages over to his wife on Saturday night and allows her to apportion all expenditures," in fact how much the wife received from the husband's wages and what he kept back "depends on the personal adjustment between them and not on a recognized rule" (Anthony 1914, pp. 135-36). Evidence on precisely how the money was allocated is very limited. But the West Side study suggests that the outcome was usually rigged in favor of the husband. As one Italian wife explained: "Of course they don't give all they make. They're men and you never know their ways" (Odencrantz 1919, p. 176). A study of unskilled Chicago wage earners in 1924 found that, when asked about their husbands' weekly earnings, over two-thirds of the wives gave lesser amounts than the actual earnings found on the payroll. The investigator concluded that the man "may not give his entire earnings to his wife, but may simply give her the amount he thinks she should spend for the family" (Houghteling 1927, p. 37).

Thus, the idealized view of a solidary family economy coordinated and controlled by the wife concealed competing internal claims for money. The husband's pay envelope was not always intact on arrival. Neither were the children's. Tantalized by the attractions of a consumer culture, children increasingly withheld or manipulated their earnings. David Nasaw (1985, pp. 131-32) found that, in the early part of this century, wage-earning children "who were obedient in every other regard did what they had to to preserve some part of their earnings for themselves. They lied, they cheated, they hid away their nickels and dimes, they doctored their pay envelopes" (see also Zelizer 1987, pp. 97-112). While working girls were more likely than their brothers to hand their wages over intact, not all of them did. Italian working girls on the New York West Side told investigators how easy it was to "knock down" a paycheck when they made overtime: "Whatever you make is written outside in pencil. . . . That's easy to fix—you have only to rub it out, put on whatever it usually is, and pocket the change" (True 1914, p. 49; on the

Money

increased individualization of children's income, especially after the 1920s, see Smith 1985; Ewen 1985).

Even the portion of money that the wife did receive and control was limited to housekeeping money. As with wealthier women, the working-class wife had no right and much less access to a personal fund. Pocket money for personal expenses was a male prerogative or a working child's right. The working-class husband's allowance was thus a very different kind of money from the allowance of middle-class wives. Although partly allocated for useful expenses, food or clothing or transportation, it was also a legitimate fund for personal pleasures. Indeed, Kathy Peiss's (1986) study of leisure among working-class women in turn-of-the-century New York clearly shows that while men could afford to pay for their amusements, drinking in saloons, attending movies and the theater, or buying tobacco, their wives had no money left for personal recreation. Thus, women's money retained a collective identity, while men's and children's money was differentiated and individualized.¹³

As home-economics experts began to encourage joint control of the domestic dollar, the working-class allowance system lost its legitimacy. Studies of English working-class families suggest that there was a shift to the middle-class system of housekeeping allowances for wives (Oren 1973, p. 115; Sterns 1972, p. 116; Pahl 1980, pp. 332–33). Limited data make it difficult to determine whether the same was true for the United States. In the 1920s, when the Lynds (1956, p. 127, n. 24) studied Muncie, Indiana, they reported that it was rare for a husband to turn over his paycheck and allow his wife control over the household economy (see also Friend 1930, p. 108). But class differences seem to have persisted; by 1938, according to the *Ladies Home Journal* national survey on money, only 38% of women in income groups under \$1,500 received an allowance, compared with 62% of those in groups over \$1,500 (Pringle 1938, p. 102).

Pin Money versus Real Money: Defining Women's Earnings

What happened when women's money did not come from their husbands' paychecks? When women worked for nonrelatives, whether at home or

¹³ If a working-class wife needed more money, her options were limited. With little access to credit accounts, she often turned to pawnbrokers and moneylenders (see Ross 1982, p. 590; Tebbutt 1983; Ayers and Lambertz 1986, pp. 203–4). Sometimes women relied on their younger children for extra cash. During a government investigation of industrial home work conducted in 1918 ("Industrial Home Work of Children" 1924, p. 22), one mother explained that her little boy helped her wire rosary beads at home because she needed "some money of her own." Another mother needed false teeth and "thought the children might just as well help to buy them."

for wages, the boundary between that income and real money was still preserved, only in different ways. In the working class, for instance, a married woman's income, usually earned by caring for boarders, taking in sewing or laundry, or, among farm families, by selling butter, eggs, or poultry, did not have the same visibility as her husband's paycheck (Jensen 1980; Ulrich 1983, pp. 45–47; Morawska 1985, pp. 134–35). As her labor was part of a woman's traditional repertoire of domestic tasks, the money she made was merged into the family's housekeeping money and usually spent on home and family, for clothing or food. Legally, in fact, until the early decades of the 20th century, those domestic earnings belonged to the husband. And the courts staunchly opposed converting a wife's money into her tangible property.¹⁴ In a growing number of personal injury cases, where the law had to decide whether the husband or the wife was entitled to recover for a woman's inability to work, as well as in claims brought up by creditors, the courts insisted on distinguishing between the domestic dollar and an earned wage. If a wife worked at home, even if her labor was performed for strangers, caring for a boarder or nursing a neighbor, that money was not a real earning and therefore belonged to her husband. Ironically but significantly, in some states a wife's domestic earnings could become her property but only as her husband's gift (Thorton 1900; Rodgers 1902; Warren 1925).

Thus, earned domestic money, much like the allowance, retained a separate identity as a gift, not as real money. Money earned by married women in the labor force was also special and different. It even had its own name. The term "pin money," which in 17th-century England had meant a separate, independent income for a wife's personal use—and was included as a formal clause in upper-class marriage contracts—lost its elitist British origins in turn-of-the-century America and now meant the supplementary household income earned by wives (Stone 1977, p. 244; Gore 1834). Still it was treated as a more frivolous, less serious earning than the husband's. As a 1903 article in *Harper's Bazaar* aptly remarked: "No man works for pin-money. The very idea makes one smile" (Leonard 1903, p. 1060).

The boundary between women's earned income and the husband's salary was also marked by their differential uses. John Modell (1978, p. 225), for instance, suggests that among late 19th-century, native-born American families, "all dollars were not equal" and women's income (as

¹⁴ Starting in the mid-19th century, Married Women's Property Acts granted wives the right to own and control their property but focused primarily on inherited property. Married women's rights to their earnings were excluded by the acts and were incorporated only slowly and with much resistance by amendments or in later statutes (see Edwards 1893; Rodgers 1902; Warren 1925; Crozier 1935, pp. 37–41; Shamma, Salmon, and Dahlin 1987, pp. 88–89, 96–97, 163).

well as children's) was spent differently and less freely than the husbands'. Among farm families, women's egg money and butter money were distinguished from husbands' wheat money or corn money (Thornton 1900, p. 188; Atkeson 1929).¹⁵ Jensen (1980) suggests that there existed a dual economy, with women and children providing for living expenses while husbands paid for mortgages and new machinery (see also Whitehead 1984, p. 112). For middle-class women, discreet forms of earning pin money at home (making preserves, pickles, or pound cake, knitting shawls or sweaters, or raising poultry or Angora cats) were approved, but, again, only for certain types of expenses: charity, for example, or "a daughter's lessons in music or art" (Sangster 1905, p. 32).

In the 1920s and 1930s, as more married women entered the labor force, their earnings, regardless of the sums involved, were still defined as pin money, categorized as supplementary income, used for the family's extra expenses, or earmarked by more affluent couples as discretionary "fun" money. For instance, one woman told an *Outlook* reporter that she reserved her income exclusively for buying clothes. Another explained: "We blow my money on extra trips abroad, antiques, anything extravagant." Others used their salary to pay the maid's wages and saved the rest (Smith 1928, p. 500). A story in the *Saturday Evening Post*, four years later, reported on the persistent "wife-keeps-all-theory" of wives' earnings. Couples in which the wife was employed were asked what her money was used for: "Keeps it all for herself . . . saves it, spends it, just as she likes," was a common response. "The important thing [is] . . . she mustn't help her husband out" (Ray 1932, p. 11).

CONCLUSION

Domestic money is thus a very special kind of currency. It would be difficult to understand its changing meanings, allocation, and uses in the United States between the 1870s and the 1930s without an awareness of the new cultural "code" and accompanying social changes. In the case of married women, their money was routinely set apart from real money by a complex mixture of ideas about family life, by a changing gender power

¹⁵ The relative importance of gender vs. the source of income in distinguishing between the two kinds of money remains unclear. For instance, Thomas and Znaniecki (1958, p. 165) suggested that the qualitative difference between the money a peasant got from selling a cow and the money his wife obtained from selling eggs and milk was not marked by gender but by the "different sort of value" represented by each type of money: the cow was property, while eggs and milk were income. Each type of money was set aside for different types of expenses. However, since, within the peasant economy, property belonged to a "higher economic class" than income, it is clear that gender did intervene in the social marking of the two monies; lower-value money was assigned to women.

structure, and by social class. Normative expectations of the family as a special noncommercial sphere made any overt form of market intrusion in domestic affairs not only distasteful but a direct threat to family solidarity. Thus, regardless of its sources, once money had entered the household, its allocation, calculation, and uses were subject to a set of domestic rules distinct from the rules of the market. Family money was nonfungible; social barriers prevented its conversion into ordinary wages.

But family culture did not affect its members equally. Thus, gender introduced a further type of nonmarket distinction in the domestic flow of funds: a wife's money was not the same kind of money as her husband's. When a wife did not earn wages, gender shaped many things.

1. *The allocation of her money.*—In the hierarchically structured family, husbands gave wives part of their income. To obtain additional money, wives were restricted to asking and cajoling or else stealing.

2. *The timing of this allocation.*—It either had no prescribed timing (dole method), so that to obtain money a wife had to ask each time, or it followed a weekly or monthly pattern (allowance).

3. *The uses of her money.*—Wives' money meant housekeeping money, a necessary allotment restricted to family expenses and excluding personal spending money. Pocket money was a budgetary expectation for husbands and children, but not for wives. Ironically, it appears that even shoplifting by married women was often collectivity oriented, as women stole from department stores "ribbons or laces to adorn the babies' clothes . . . or often little gifts for [their husbands]" ("Husband Who Makes His Wife a Thief" 1915).

4. *The quantity of her money.*—Wives usually received small sums of money. The amount of an allowance was not determined by the efficiency or even the quantity of a wife's domestic contributions but by prevalent beliefs of what was a proper amount. Therefore, a larger paycheck for the husband need not translate into a raise in the housekeeping allowance. On the basis of gender economics, it might in fact simply increase a husband's personal money (Oren 1973, p. 110; Land 1977).

Changes in gender roles and family structure influenced the meaning and methods of allocation of married women's money. The traditional dole or "asking" method became, as women's consumer role expanded, not only inefficient but also inappropriate in increasingly egalitarian marriages. The allowance, praised as a more equitable method of allocation in the early part of the century, was in turn condemned by home-efficiency experts of the 1920s and 1930s as an unsatisfactory payment for modern wives. The joint account emerged as the new cultural ideal. (For some recent changes in the allocation system of domestic money, see Blumstein and Schwartz [1985]; Hertz [1986]; Treas [1989].)

What about the uses of married women's money? In contrast to the

variability of allocation methods, the earmarking of a wife's housekeeping income for collective consumption remained remarkably persistent. Despite the increasing individualization of consumption patterns and the encouragement by home-economics experts to allot personal funds for each family member in the domestic budget, personal spending money for wives was still obtained by subterfuge or spent with guilt. (For some recent evidence of the enduring division between wives' collective money and husbands' personal spending money, see Wilson [1987].)

Gender marked women's money even when their income was earned. Women's wages were still earmarked as separate and treated differently. A wife's pin money, regardless of its quantity, and even when it brought the family a needed income, remained a less fundamental kind of money than her husband's wages. It was either collectivized or trivialized, merged into the housekeeping fund and thus undifferentiated from collective income or else treated as a supplementary earning designated either for family expenses (a-child's education or a vacation) or for frivolous purposes (clothing or jewelry). (For contemporary evidence on restricted uses of wives' earnings, see Hood [1983, p. 62].) The trivialization of women's earnings extended beyond the private domestic economy. For the opponents of women's labor, pin money was a socially irresponsible currency, a luxury income that threatened the wages of the real provider (see Kessler-Harris 1982, pp. 100-101). Thus, despite strong statistical evidence that pin money was often in fact a "family coupling pin, the only means of holding the family together and of making ends meet," women's earnings were systematically stigmatized as "money for trinkets and trifles" (Anderson 1929, p. 921).

But the circulation of domestic money was not shaped by gender alone. Social class added a further set of social restrictions on the liquidity of money. The middle-class method of allocating household money was reversed in the working class, where wives handed out allowances instead of receiving them. The working-class wife's managerial power was thus greater than that of her middle-class counterpart, although her discretionary power may not have differed significantly.

Domestic money thus shows the limits of a purely instrumental, rationalized model of market money, which conceals qualitative distinctions among kinds of money in the modern world.¹⁶ Domestic money is a

¹⁶ Ironically, Max Weber's own family of origin provided evidence against his rational conception of money. According to Marianne Weber (1975, p. 141), Weber's father "was typical of the husbands of the time [1860s] . . . who needed to determine by themselves how the family income was to be used and left their wives and children in the dark as to how high the income was." Helene, Weber's mother, had no housekeeping allowance, "nor a special fund for her personal needs." I thank Marta Giel for this reference.

special money, not just a medium of economic exchange but a meaningful, socially constructed currency, shaped by the domestic sphere where it circulates and by the gender and social class of its domestic “money handlers.” Age also marks domestic money. In fact, between the 1870s and 1930s, children’s money was the subject of much controversy within families and among educational experts. The allowance emerged as the proper money income for children. But it had a different meaning, method of allocation, and uses from the allowances of middle-class wives or working-class men. Closely supervised by parents, it was defined primarily as educational money, teaching children proper social and moral values, as well as consumer skills (see Zelizer 1987).

The cultural and social “life” of domestic money also challenges the allegedly irrevocable dominance of the “cash nexus” in the modern world. To be sure, Marx and Engels ([1848] 1971, p. 11) were partly correct when they accused the bourgeoisie of reducing family relations “to a mere money relation.” Money concerns did increasingly permeate the American household. In fact, in the 1920s, some observers ironically predicted that the national enthusiasm for rationalized housekeeping and budget making would turn “Home, Sweet Home” into “Home, Solvent Home,” with “Ma and Pa a couple of cash registers, and the kiddies little adding machines” (Phillips 1924, p. 15). Yet, such nightmare visions of a commercialized world failed to capture the complexity and reciprocal aspects of the phenomenon of monetization. Money came into American homes but it was transformed in the process, as it became part of the structure of social relations and the meaning system of the family.

The case of domestic money is only one example, an empirical indicator of a complex social economy that remains hidden in the dominant economic paradigm of a single, qualityless, and rationalizing market money. This article attempts to lay some of the preliminary groundwork for a richer, alternative sociological model of special monies, one that argues for a plurality of qualitatively distinct kinds of money in the modern world, with each special money shaped by different networks of social relations and varying systems of meanings. No money, including market money, escapes such extraeconomic influences. For instance, at the turn of the century, not only the domestic dollar but other kinds of monies created different yet equally significant cultural and social dilemmas for American society. “Charitable money” raised questions about the proper uses and allocation of money as a gift among strangers or between kin, whether in the form of charity, wills, “ritual” gifts (for weddings, birthdays, bar mitzvahs, and Christmas), or even tipping, while “sacred money” provoked discussions about the moral quality of money: Under what conditions did money acquire sacred religious or moral value, and when did it become dirty money, defined as collectively or individually

demeaning? Or consider the definitional problems of "institutional money." How should money be allocated, regulated, and defined in the separate social world of a prison, a mental asylum, or an orphanage?

But an inventory of monies, however revealing, provides only a descriptive catalog. To develop a social theory of money, we must then explain the sources and patterns of variation among special monies, exploring how various structures of social relations and cultural values shape and constrain the qualitative life of different monies by (a) earmarking specified uses of money, (b) regulating modes of allocation, (c) designating proper users, and (d) assigning special symbolic meanings. The concept of multiple monies thus raises fundamental questions about the characteristics of social and cultural boundaries, such as: (1) What are the origins of boundaries between monies? What, for instance, are the differences between special monies imposed by a central authority (as in a prison) and monies defined through social interaction (as in the family)? (2) How do the social statuses of transactors affect the formation of special monies? (3) What determines the relative rigidity or permeability of boundaries between special monies? (4) What are the patterns of conversions between special monies? (5) Is there a moral ranking between special monies, as has been suggested for primitive monies (see Bohannan 1959)? (6) When and how do boundaries between special monies break down?

Developing a sociological model of multiple monies is part of a broader challenge to neoclassical economic theory. It offers an alternative approach not only to the study of money but to all other aspects of economic life, including the market (see Zelizer 1988). As I show in this article, economic processes of exchange and consumption are one special category of social relations, much as is kinship or religion. Thus, economic phenomena such as money, although partly autonomous, are interdependent with historically variable systems of meanings and structures of social relations.

REFERENCES

Abel, Mary W. 1921. *Successful Family Life on Moderate Income*. Philadelphia: Lippincott.
 "Adventures in Economic Independence." 1915. *Harper's Weekly* 61:610.
 Anderson, Mary. 1929. *United States Daily*, September 23. Cited in editorial, *Journal of Home Economics* 21 (December): 920-22.
 Anthony, Katherine. 1914. *Mothers Who Must Earn*. New York: Survey.
 Appadurai, Arjun, ed. 1986. *The Social Life of Things*. Cambridge: Cambridge University Press.
 Atkeson, Mary M. 1919. "Women in Farm Life and Rural Economy." *Annals of the American Academy of Political and Social Science* 143:188-94.
 Ayers, Pat, and Jan Lambertz. 1986. "Marriage Relations, Money and Domestic

American Journal of Sociology

Violence in Working-Class Liverpool, 1919–39.” Pp. 195–219 in *Labour and Love*, edited by Jane Lewis. Oxford: Blackwell.

Banner, Lois W. 1983. *American Beauty*. Chicago: University of Chicago Press.

Barber, Bernard. 1977. “The Absolutization of the Market: Some Notes on How We Got from There to Here.” Pp. 15–31 in *Markets and Morals*, edited by G. Dworkin, G. Bermant, and P. Brown. Washington, D.C.: Hemisphere.

Baric, Lorraine. 1964. “Some Aspects of Credit, Saving and Investment in a Non-Monetary Economy (Rossel Island).” Pp. 35–42 in *Capital, Saving and Credit in Peasant Societies*, edited by Raymond Firth and B. S. Yamey. Chicago: Aldine.

Beecher, Catharine E. 1841. *A Treatise on Domestic Economy*. Boston: Marsh, Capen, Lyon & Webb.

Benson, Susan Porter. 1986. *Counter Cultures*. Champaign: University of Illinois Press.

Black, Hugh. 1921. “Money and Marriage.” *Delineator* 98:58.

Blood, Robert O., Jr., and Donald M. Wolfe. 1965. *Husbands and Wives*. New York: Free Press.

Blumstein, Philip, and Pepper Schwartz. 1985. *American Couples*. New York: Pocket.

Bodnar, John. 1987. *The Transplanted*. Bloomington: Indiana University Press.

Bohannan, Paul. 1959. “The Impact of Money on an African Subsistence Economy.” *Journal of Economic History* 19:491–503.

Brown, Norman O. 1959. *Life against Death*. Middletown, Conn.: Wesleyan University Press.

Byington, Margaret F. 1910. *Homestead*. New York: Charities Publication Committee.

Cheal, David. 1988. *The Gift Economy*. London: Routledge & Kegan Paul.

Child, Maude Parker. 1926. “Her Weight in Gold.” *Saturday Evening Post* 198:125.

Clark, Homer H., Jr. 1968. *The Law of Domestic Relation in the United States*. St. Paul, Minn.: West.

Collins, Randall. 1979. Review of *The Bankers*, by Martin Mayer. *American Journal of Sociology* 85:190–94.

Cooley, Charles H. 1913. “The Sphere of Pecuniary Valuation.” *American Journal of Sociology* 19:188–203.

Cott, Nancy F. 1977. *The Bonds of Womanhood*. New Haven: Yale University Press.

Cowan, Ruth S. 1983. *More Work for Mother*. New York: Basic.

Crozier, Blanche. 1935. “Marital Support.” *Boston University Law Review* 15:33.

Crump, Thomas. 1981. *The Phenomenon of Money*. London: Routledge & Kegan Paul.

Delphy, Christine, and Diana Leonard. 1986. “Class Analysis, Gender Analysis and the Family.” Pp. 57–73 in *Gender and Stratification*, edited by Rosemary Crompton and Michael Mann. Oxford: Polity.

Di Leonardo, Micaela. 1984. *The Varieties of Ethnic Experience*. Ithaca: Cornell University Press.

Dix, Dorothy. 1914. “Woman and Her Money.” *Good Housekeeping* 58:408–9.

Douglas, Mary. 1967. “Primitive Rationing.” Pp. 119–45 in *Themes in Economic Anthropology*, edited by Raymond Firth. London: Tavistock.

Douglas, Mary, and Baron Isherwood. 1979. *The World of Goods*. New York: Norton.

Edwards, Percy. 1893. “Is the Husband Entitled to His Wife’s Earnings?” *Canadian Law Times* 13:159–76.

Einzig, Paul. 1966. *Primitive Money*. Oxford: Pergamon.

Ewen, Elizabeth. 1985. *Immigrant Women in the Land of Dollars*. New York: Monthly Review.

“Family Pocketbook.” 1910. *Good Housekeeping* 51:9–15.

Frederick, Christine. 1919. *Household Engineering*. Chicago: American School of Home Economics.

Money

_____. 1929. *Selling Mrs. Consumer*. New York: Business Bourse.

Friend, Mata R. 1930. *Earning and Spending Family Income*. New York: Appleton.

Geertz, Clifford. 1973. "Deep Play: Notes on the Balinese Cockfight." Pp. 412–53 in *The Interpretation of Cultures*, edited by Clifford Geertz. New York: Basic.

Goffman, Erving. 1961. *Asylums*. Garden City, N.Y.: Anchor.

Gore, Catherine. 1834. *Pin-Money*. Boston: Allen & Ticknor.

Granovetter, Mark. 1985. "Economic Action and Social Structure: The Problem of Embeddedness." *American Journal of Sociology* 91:481–510.

Gullestad, Marianne. 1984. *Kitchen-Table Society*. New York: Columbia University Press.

Hamilton, G. V., and Kenneth MacGowan. 1928. "Marriage and Money." *Harper's Monthly* 157:434–44.

Hartmann, Heidi I. 1981. "The Family as the Locus of Gender, Class, and Political Struggle: The Example of Housework." *Signs* 6:366–94.

Hertz, Rosanna. 1986. *More Equal than Others*. Berkeley and Los Angeles: University of California Press.

Hirschman, Albert O. 1986. *Rival Views of Market Society*. New York: Viking.

Hood, Jane. 1983. *Becoming a Two-Job Family*. New York: Praeger.

Horowitz, Daniel. 1985. *The Morality of Spending*. Baltimore: Johns Hopkins University Press.

Houghteling, Leila. 1927. *Income and Standard of Living of Unskilled Laborers in Chicago*. Chicago: University of Chicago Press.

"Husband Who Makes His Wife a Thief." 1915. *Ladies Home Journal* 32:16.

"Industrial Home Work of Children." 1924. U.S. Department of Labor, Children's Bureau Publication no. 100, Washington, D.C.

Ives, Alice. 1890. "The Domestic Purse Strings." *Forum* 10:106–14.

Jensen, Joan M. 1980. "Cloth, Butter and Boarders: Women's Household Production for the Market." *Review of Radical Political Economics* 12:14–24.

_____. 1986. *Loosening the Bonds*. New Haven: Yale University Press.

Kahneman, Daniel, and Amos Tversky. 1982. "The Psychology of Preferences." *Scientific American* 246:160–73.

Kelland, Clarence Budington. 1928. "Wives Are Either Tightwads or Spendthrifts." *American Magazine* 106:12, 104–10.

Kessler-Harris, Alice. 1982. *Out to Work*. New York: Oxford University Press.

Komarovsky, Mirra. 1961. "Class Differences in Family Decision-Making on Expenditures." Pp. 255–65 in *Household Decision-Making*. Vol. 4. of *Consumer Behavior*. Edited by Nelson N. Foote. New York: New York University Press.

_____. 1967. *Blue-Collar Marriage*. New York: Vintage.

Kopytoff, Igor. 1986. "The Cultural Biography of Things: Commoditization as Process." Pp. 64–91 in *The Social Life of Things*, edited by Arjun Appadurai. Cambridge: Cambridge University Press.

Kyrk, Hazel. 1933. *Economic Problems of the Family*. New York: Harper.

Lamphere, Louise. 1986. "From Working Daughters to Working Mothers: Production and Reproduction in an Industrial Community." *American Ethnologist* 13:118–30.

Land, Hilary. 1977. "Inequalities in Large Families: More of the Same or Different?" Pp. 163–76 in *Equalities and Inequalities in Family Life*, edited by Robert Chester and John Peel. New York: Academic.

Lea, Stephen E. G., Roger Tarpy, and Paul Webley. 1987. *The Individual in the Economy*. New York: Cambridge University Press.

Leonard, Priscilla. 1903. "Pin Money versus Moral Obligations." *Harper's Bazaar* 37:1060.

Letters to the Editor. *Good Housekeeping* 51 (February 1910): 245.

Leupp, Frances E. 1909. "The Cooperative Family." *Atlantic Monthly* 104:762–67.

Luxton, Meg. 1980. *More than a Labour of Love*. Toronto: Women's Press.

American Journal of Sociology

Lynd, Robert S. 1932. "Family Members as Consumers." *Annals of the American Academy of Political and Social Science* 160:86–93.

Lynd, Robert S., and Helen Merrell Lynd. 1956. *Middletown*. New York: Harcourt Brace.

Marx, Karl. (1844) 1964. "The Power of Money in Bourgeois Society." In *The Economic and Philosophic Manuscripts of 1844*. New York: International.

_____. (1858) 1972. *A Contribution to the Critique of Political Economy*, edited by Maurice Dobb. New York: International.

_____. (1858–59) 1973. *Grundrisse*. New York: Vintage.

_____. (1867) 1984. *Capital*, vol. 1. Edited by Friedrich Engels. New York: International.

Marx, Karl, and Friedrich Engels. (1848) 1971. *The Communist Manifesto*. New York: International.

May, Elaine Tyler. 1980. *Great Expectations*. Chicago: University of Chicago Press.

Mazeaud, Henri, Léon Mazeaud, and André Tunc. 1957. *Traité théorique et pratique de la responsabilité civile délictuelle et contractuelle*, 5th ed. Paris: Editions Montchrestien.

Melitz, Jacques. 1970. "The Polanyi School of Anthropology on Money: An Economist's View." *American Anthropologist* 72:1020–40.

Messinger, C. S., et al. 1890. "Shall Our Daughters Have Dowries?" *North American Review* 151:746–69.

Miller, Daniel. 1987. *Material Culture and Mass Consumption*. Oxford: Blackwell.

Mirowsky, John. 1985. "Depression and Marital Power: An Equity Model." *American Journal of Sociology* 91:557–92.

Modell, John. 1978. "Patterns of Consumption, Acculturation and Family Income: Strategies in Late Nineteenth-Century America." Pp. 206–40 in *Family and Population in Nineteenth-Century America*, edited by Tamara K. Hareven and Maris A. Vinovskis. Princeton: Princeton University Press.

Morawska, Ewa. 1985. *For Bread with Butter*. Cambridge: Cambridge University Press.

More, Louise Bolard. 1907. *Wage-Earners' Budgets*. New York: Holt.

Nasaw, David. 1985. *Children of the City*. New York: Anchor.

Norton, Mary Beth. 1979. "Eighteenth-Century American Women in Peace and War." Pp. 136–61 in *A Heritage of Her Own*, edited by Nancy F. Cott and Elizabeth H. Pleck. New York: Simon & Schuster.

Odencrantz, Louise C. 1919. *Italian Women in Industry*. New York: Russell Sage.

O'Hagan, Anne. 1909. "The Little Foxes." *Good Housekeeping* 49:369–74.

Oren, Laura. 1973. "The Welfare of Women in Laboring Families: England, 1860–1950." *Feminist Studies* 1:107–23.

Ostrander, Susan A. 1984. *Women of the Upper Class*. Philadelphia: Temple University Press.

Pahl, Jan. 1980. "Patterns of Money Management within Marriage." *Journal of Social Policy* 9:313–35.

Parsons, Talcott. 1967. "On the Concept of Influence." Pp. 355–82 in *Sociological Theory and Modern Society*. New York: Free Press.

_____. 1971a. "Higher Education as a Theoretical Focus." In *Institutions and Social Exchange*, edited by Herman Turk and Richard L. Simpson. New York: Bobbs-Merrill.

_____. 1971b. "Levels of Organization and the Mediation of Social Interaction." In *Institutions and Social Exchange*, edited by Herman Turk and Richard L. Simpson. New York: Bobbs-Merrill.

Parsons, Talcott, and Neil J. Smelser. 1956. *Economy and Society*. New York: Free Press.

Money

Peattie, Elia W. 1911. "Your Wife's Pocketbook." *Delineator* 77:466.

Peiss, Kathy. 1986. *Cheap Amusements*. Philadelphia: Temple University Press.

Phillips, H. I. 1924. "My Adventures as a Bold, Bad Budgeteer." *American Magazine* 97:64.

Polanyi, Karl. 1957. "The Economy as an Instituted Process." Pp. 243–70 in *Trade and Market in the Early Empires*, edited by Karl Polanyi, Conrad M. Arensberg, and Harry W. Pearson. Glencoe, Ill.: Free Press.

Post, Emily. 1928. "Kelland Doesn't Know What He Is Talking About." *American Magazine* 106:110.

Pringle, Henry F. 1938. "What Do the Women of America Think about Money?" *Ladies Home Journal* 55:102.

Radin, Margaret. 1987. "Market Inalienability." *Harvard Law Review* 100:1849–1937.

Rainwater, Lee. 1974. *What Money Buys*. New York: Basic.

Ray, Mary Beynon. 1932. "It's Not Always the Woman Who Pays." *Saturday Evening Post* 205:15, 48–49.

Reddy, William. 1984. *The Rise of Market Culture: The Textile Trade and French Society, 1750–1900*. Cambridge: Cambridge University Press.

———. 1987. *Money and Liberty in Modern Europe*. New York: Cambridge University Press.

Rodgers, Helen Z. M. 1902. "Married Women's Earnings." *Albany Law Journal* 64:384.

Ross, Ellen. 1982. "'Fierce Questions and Taunts': Married Life in Working Class London, 1870–1914." *Feminist Studies* 8:576–602.

Rubin, Lillian B. 1976. *Worlds of Pain*. New York: Basic.

Ryan, Mary P. 1984. *Cradle of the Middle Class*. New York: Cambridge University Press.

Safilios-Rothschild, Constantina. 1970. "The Study of Family Power Structure." *Journal of Marriage and the Family* 32:539–52.

Sahlins, Marshall. 1976. *Culture and Practical Reason*. Chicago: University of Chicago Press.

Salmon, Lucy M. 1909. "The Economics of Spending." *Outlook* 91:884–90.

Salmon, Marylynn. 1986. *Women and the Law of Property*. Chapel Hill: University of North Carolina Press.

Sangster, Margaret E. 1905. "Shall Wives Earn Money?" *Woman's Home Companion* 32:32.

Schudson, Michael. 1984. *Advertising, the Uneasy Persuasion*. New York: Basic.

Seeley, John R., R. Alexander Sim, and Elizabeth W. Loosley. 1956. *Crestwood Heights*. New York: Wiley.

Sen, Amartya. 1983. "Economics and the Family." *Asian Development Review* 1:14–26.

Shammas, Carole, Marylynn Salmon, and Michel Dahlin. 1987. *Inheritance in America*. New Brunswick: Rutgers University Press.

Simiand, François. 1934. "La monnaie, réalité sociale." *Annales Sociologiques*, ser. D, pp. 1–86.

Simkhovitch, Mary K. 1917. *The City Worker's World in America*. New York: Macmillan.

Simmel, Georg. (1900) 1978. *The Philosophy of Money*, translated by Tom Bottomore and David Frisby. London: Routledge & Kegan Paul.

———. (1908) 1950. *The Sociology of Georg Simmel*, edited by Kurt H. Wolf. Glencoe, Ill.: Free Press.

Smelt, Simon. 1980. "Money's Place in Society." *British Journal of Sociology* 31:205–23.

American Journal of Sociology

Smith, Helena Huntington. 1928. "Husbands, Wives, and Pocketbooks." *Outlook*, p. 500.

Smith, Judith E. 1985. *Family Connections*. Albany: State University of New York Press.

Sorokin, Pitirim A. 1943. *Sociocultural Causality, Space, Time*. Durham, N.C.: Duke University Press.

Stansell, Christine. 1986. *City of Women*. New York: Knopf.

Stearns, Peter N. 1972. "Working-Class Women in Britain, 1890–1914." Pp. 100–120 in *Suffer and Be Still*, edited by Martha Vicinus. Bloomington: Indiana University Press.

Stone, Lawrence. 1977. *The Family, Sex and Marriage*. New York: Harper & Row.

Sugimoto, Etsu Inagaki. (1926) 1936. *A Daughter of the Samurai*. Garden City, N.Y.: Doubleday.

Sullerot, Evelyn. 1966. "Les femmes et l'argent." *Janus* 10:33–39.

Sumner, William Graham. (1906) 1940. *Folkways*. New York: Mentor.

Swedberg, Richard. 1987. "Economic Sociology: Past and Present." *Theory and Society* 16:169–213.

Taussig, Michael. 1980. *The Devil and Commodity Fetishism in South America*. Chapel Hill: University of North Carolina Press.

Tebbutt, Melanie. 1983. *Making Ends Meet: Pawnbroking and Working-Class Credit*. New York: St. Martin's.

Tentler, Leslie W. 1982. *Wage-earning Women*. New York: Oxfcrd University Press.

Tilly, Louise A., and Joan W. Scott. 1978. *Women, Work, and Family*. New York: Holt, Rinehart & Winston.

Thaler, Richard. 1985. "Mental Accounting and Consumer Choice." *Marketing Science*, vol. 4 (Summer).

Thomas, W. I., and Florian Znaniecki. (1918–20) 1958. *The Polish Peasant in Europe and America*. New York: Dover.

Thornton, W. W. 1900. "Personal Services Rendered by Wife to Husband under Contract." *Central Law Journal* 183.

Treas, Judith. 1989. "Money in the Bank: Transaction Costs and Privatized Marriage." *American Sociological Review*, in press.

True, Ruth S. 1914. *The Neglected Girl*. New York: Survey.

Turner, Bryan S. 1986. "Simmel, Rationalisation and the Sociology of Money." *Sociological Review* 34:93–114.

Ulrich, Laurel Thatcher. 1983. *Good Wives*. New York: Oxford University Press.

Veblen, Thorstein. (1899) 1953. *The Theory of the Leisure Class*. New York: Mentor.

Warren, Joseph. 1925. "Husband's Right to Wife's Services." *Harvard Law Review* 38:421.

W.A.S. 1922. "Charge It to My Husband." *Law Notes* 26:26–28.

Weber, Marianne. 1975. *Max Weber: A Biography*. New York: Wiley.

Weber, Max. (1922) 1978. *Economy and Society*, edited by Guerther Roth and Claus Wittich. Berkeley: University of California Press.

—. (1946) 1971. "Religious Rejections of the World and Their Directions." Pp. 323–59 in *From Max Weber: Essays in Sociology*, edited by H. H. Gerth and C. Wright Mills. New York: Oxford University Press.

Weitzman, Lenore J. 1981. *The Marriage Contract*. New York: Free Press.

Welter, Barbara. 1966. "The Cult of True Womanhood: 1820–1850." *American Quarterly* 18:151–74.

Whitehead, Ann. 1984. "'I'm Hungry, Mum.'" Pp. 93–116 in *Of Marriage and the Market*, edited by Kate Young, Carol Wolkowitz, and Roslyn McCullagh. London: Routledge & Kegan Paul.

Wilson, Gail. 1987. *Money in the Family*. Brookfield, Vt.: Gower.

Winter, Alice Ames. 1925. "The Family Purse." *Ladies Home Journal* 42:185.

Money

Wong, Diana. 1984. "The Limits of Using the Household as a Unit of Analysis." Pp. 56–63 in *Households and the World-Economy*, edited by Joan Smith, Immanuel Wallerstein, and Hans-Dieter Evans. Beverly Hills, Calif.: Sage.

Young, Michael. 1952. "Distribution of Income within the Family." *British Journal of Sociology* 3:305–21.

Zelizer, Viviana. 1987. *Pricing the Priceless Child: The Changing Social Value of Children*. New York: Basic.

_____. 1988. "Beyond the Polemics on the Market: Establishing a Theoretical and Empirical Agenda." *Sociological Forum* 3 (Fall): 614–34.

Occupational Status-Assignment Systems: The Effect of Status on Self Esteem¹

William A. Faunce
Michigan State University

It is commonly assumed that work necessarily affects self esteem, but the research evidence does not support this assumption. Instead, evidence suggests a wide variation in the relationship between occupational achievement and self assessment. This article proposes an explanation for this variation based on the postulate that the effects of status on self esteem occur not simply as a result of knowledge of one's location in a status hierarchy but, more important, as a result of the frequency with which one is reminded of that location. If this postulate is correct, it becomes important to identify variables that call attention to occupational achievement. This article introduces the concept of the "status-assignment system" and discusses structural characteristics of such systems that produce reminders of status location.

In this article, I examine some social-structural variables that can be expected to influence the relationship between occupational status and self esteem.² Granovetter (1985) has argued that, to avoid both an oversocialized and an undersocialized conception of economic action, we must attend to the embeddedness of economic actions in the structure of social relations. Stryker and Macke (1978) conclude from a review of studies of

¹ I would like to thank Bo Anderson, Peter Manning, and Anne McMahon for their comments on an earlier version of this article. Requests for reprints should be sent to William A. Faunce, Department of Sociology, Michigan State University, East Lansing, Michigan 48824.

² The hyphen between "self" and "esteem" has been omitted here and is omitted in this article from most other expressions containing the term "self." The grammatical convention requiring this hyphen emphasizes the reflexive character of the self. My concern, however, is almost exclusively with the self as object, and, where this is the case, there seems no more reason to hyphenate an expression such as "self attitudes" than to hyphenate expressions such as "work attitudes," "family attitudes," or any others where the modifier simply designates the object of the attitude. By avoiding the routine use of the hyphen following the term "self," I preserve the option of using it to point out the reflexive process involved in such expressions as "self-starting" or "self-aggrandizing," where the referent for the word "self" is the actor rather than the set of attitudes the actor holds about him- or herself.

status inconsistency and role conflict that the behavioral consequences of these conditions are necessarily linked to the interaction situations in which they occur. I present a similar argument here. My position is that the consequences for self esteem of differing locations in an occupational status hierarchy cannot be fully understood without taking into account the extent to which occupational status differences enter into the social experiences of people, both in and out of the workplace. These concerns fit with a growing body of literature focused on the social-structural context of self-related processes (Burke 1980; McCall and Simmons 1978; Rosenberg 1979; Stryker 1980; Turner 1978).

While it is commonly assumed that occupational status has an important effect on self esteem, research bearing on this assumption has produced equivocal results. The interpretation of these findings that seems most consistent with the evidence is that there is considerable variation in the significance of work-related values for self esteem maintenance. The idea that work may be more or less consequential for self esteem fits a conception of the self as composed of identities and traits ordered in terms of their relative effects on self esteem. Real progress has been made recently in conceptualizing the organization of the self with regard to the relative importance of its constituent parts (Hoelter 1986; Marsh 1986; Rosenberg 1979; Stryker 1980; Stryker and Serpe 1982; Tesser 1986; Turner 1978). We have made less progress in explaining this variation. One of the missing elements is a conceptual framework for examining the social-structural *constraints* on our ability to choose bases for self evaluation. Most discussions of differences in the effect of components of the self on self esteem imply that we have a relatively free hand in selecting the criteria by which we evaluate ourselves. My primary objective here is to introduce and explicate the concept of the "status-assignment system." I will argue that the effects of status on self esteem occur not simply as a result of *knowledge* of one's location in a status hierarchy but, more important, as a result of the frequency with which one is *reminded* of that location. Variable attributes of the structure of occupational status-assignment systems determine the frequency with which occupational achievement level becomes relevant to social experience and, as a consequence, constrain our ability to "decide" whether work affects how we feel about ourselves. I will discuss examples of structural characteristics of occupational status-assignment systems that could be expected to have this effect.³

³ The term "status," as it is used in this article, refers to social honor. It means, specifically, a location in a hierarchy resulting from the unequal distribution of *anything* that is valued and that produces relations of deference, acceptance, or derogation. It therefore involves a wide range of potential characteristics, including both activities, such as type of work or familial role, and personal attributes, such as skill or

American Journal of Sociology

While the focus here is on occupational status, the processes described are seen as a special case of the general relationship between status assignment and self esteem maintenance systems. The structural conditions that make occupational status assignment relevant to evaluation of self in terms of occupational achievement should apply to the way in which any status characteristic enters into these two types of evaluative processes. The relationship between status assignment and self esteem maintenance is a critical element in the links between personality and social structure and between individuals and collectivities. The degree of correspondence between the values used by individuals in evaluating themselves and those used by others in the determination of the individual's social status is one indicator of how much the individual is an alienated or a committed participant in any social activity and should go far toward explaining the extent to which individuals are motivated to achieve collective goals. In this broader context, the argument I will present here attempts to specify the effect of social-structural conditions on sociopsychological processes related to the maintenance of self esteem.

WORK AND SELF

The conventional wisdom regarding work and self esteem is that the former is a major determinant of the latter. Everett Hughes's assertion that "a man's work is one of the things by which he is judged and certainly one of the more significant things by which he judges himself" (Hughes 1958, p. 42) is echoed throughout the literature on work attitudes. If this assertion is correct, there should be a strong association between occupational achievement and self attitudes. The evidence does not show this association. On the basis of a review of 48 studies, Wylie (1979, p. 115) concludes that there are "contradictory, weak, mostly null results regarding the relationship of socioeconomic level and over-all self-regard" and that "psychologists, sociologists, and educators have unhesitatingly and prematurely assumed that socioeconomic level would be a powerful variable in affecting the self-concept—especially over-all self-regard." Similarly, according to Kaplan (1971, p. 42), the hypothesis that persons in the lower socioeconomic strata will experience low self esteem "tends to persist in the literature despite the fact that the bulk of relevant

physical appearance. This conception of status also means that *any* type of occupational achievement locates the person in some occupational status hierarchy. I will therefore use the terms "occupational status" and "occupational achievement level" interchangeably.

studies reports little significant support for the hypothesis regardless of study group composition or indices of self-attitudes and class employed."⁴

Most studies use general socioeconomic standing as the only measure of occupational achievement and consequently fail to take account of many potential work-related self-assessments (e.g., "good provider" or "best salesman in the shop"). Elsewhere, I have referred to the practice of ignoring these finer-grained status distinctions as "chain saw sociology" (Faunce 1982). However, even if our measures of occupational status captured all the subtleties and complexities in the assessment of occupational achievement, other evidence indicates that we would still find considerable variation in the effect of occupational achievement on self esteem. On the basis of the assumption that anything affecting one's self esteem will be regarded as important, the substantial evidence showing variation in the perceived importance of work suggests variation in the effect of work on self (Maurer, Vredenburgh, and Smith 1981; Dubin, Hedley, and Taveggia 1976; Rabinowitz and Hall 1977). In addition to this inferential evidence, a few studies focus directly on the significance of work for self esteem, and these studies also show considerable variation in this relationship (Kahl 1965; Rosenberg and Pearlin 1978; Wilensky 1966).

The relative importance of identities in the self concept has been referred to as psychological centrality (Rosenberg 1979), ego involvement (Vroom 1962), identity salience (Stryker 1968, 1980), commitment (Zetterberg 1966), relevance (Tesser 1986), role-person merger (Turner 1978), and identity prominence (McCall and Simmons 1978). There are differences in meaning among these terms, but all suggest an ordering of components of the self concept in terms of their importance, and some include effect on self esteem as an explanation for this ordering. The general process through which activities or attributes acquire differential significance for self esteem will be referred to here as *selective self investment* (Faunce and Dubin 1975; Faunce 1982, 1984). *Self investment* is defined as a commitment to achievement with regard to an activity or

⁴ There are some studies showing a nontrivial, positive relationship between socioeconomic status and self esteem (Kohn and Schooler 1983; Rosenberg and Pearlin 1978; Kaplan 1971). There is also strong evidence that whatever link there may be between socioeconomic status and self esteem is mediated by such occupational characteristics as autonomy that vary with occupational status (Kohn and Schooler 1983; Mortimer and Lorence 1979; Mortimer and Finch 1986). The position I take here is not that occupational achievement is irrelevant for self evaluation, but rather that the extent of its influence will be determined by social-structural conditions affecting the frequency with which occupational status becomes the focus of attention in social encounters. Under some circumstances, I would expect work to be an important determinant of self esteem, while under others I would expect it to have no effect at all.

attribute based on the relevance of that activity or attribute for self esteem. High self investment in work, for example, means that occupational achievement in some form is necessary to maintain self esteem; those with low self investment can fail in this area with impunity.

Most discussions of selectivity among criteria for evaluation of self have treated it as a cognitive process involving considerable latitude of choice and have given limited attention to constraints on the selection process. Rosenberg, for example, emphasizes the freedom of choice individuals have and, while noting some limits, argues that wide latitude in selection of self-values exists "because of the enormous range of alternatives available and the private nature of self-values" (Rosenberg 1967, p. 35). There is evidence that we tend to value those activities or attributes that produce positive self evaluations while dismissing as unimportant activities or attributes in which we are unsuccessful (Rosenberg 1965; Tesser 1986; Hoelter 1983; Faunce 1984). It appears, however, that some failings are more difficult to ignore than others (Hoelter 1986), and there are even conditions under which people apparently seek social feedback verifying *negative* self-assessments (Swann and Read 1981; Schrauger 1975). The relationship between achievement level and self investment is complex, and, while the research clearly documents variability in the level of self investment in work, the sources of this variation remain unclear.

One potential explanation involves the process of causal attribution. An activity or attribute will influence self esteem only if individuals see themselves as personally responsible for their success or failure with respect to it. There is an extensive body of research dealing with the self-serving bias in the process of causal attribution (Bradley 1978; Miller and Ross 1975; Tetlock and Levi 1982; Weiner 1986), and, although some of its results may have alternative interpretations (Miller and Ross 1975), the evidence generally indicates that, whenever possible, we take credit for our successes and blame external circumstances for our failures. The issue of possible limits on our ability to make this choice has received much less attention. In one of the earliest discussions of attribution, Heider (1958) suggested that a causal attribution must be plausible to be acceptable: "What is selected as acceptable cause is not just anything that fits with the personal needs and wishes of the life space. It also has to fit the cognitive expectations about connections between motives, attitudes and behavior, etc." (pp. 172-73). While it seems reasonable to suppose that social-structural conditions influence the plausibility of an external attribution of causes for failure, the available research evidence does not explain this relationship. Conditions that force acceptance of personal responsibility for one's occupational achievement level can be expected to

increase self investment in work and will be included in my discussion of status-assignment systems.

A general principle regarding selectivity in the self investment process is expressed in the following postulate: The greater the frequency of self evaluation in terms of an activity or attribute, the higher will be the self investment in that activity or attribute. This postulate asserts, for example, that the more often we think about our level of occupational achievement, the greater will be the effect of work-related values on self esteem. Frequent evaluation of self on any dimension may result from various conditions, one of which is certainly frequent evaluation by others. If frequent self evaluation leads to self investment and if social evaluation generates self evaluation, we can derive the following theorem: The greater the frequency of evaluation by others of an attribute or activity, the higher will be the self investment in that activity or attribute. I will refer to this statement as the "frequency of social evaluation theorem."

Being evaluated by others, however, is not the only way, and probably not even the most common way, in which social experience engenders self evaluation. Simply being in the presence of someone who is higher or lower than we are in some status hierarchy is likely to direct our attention to our own location in that hierarchy (Rosenberg 1979; Tesser 1986). The ways in which we differ from others are especially likely to induce social comparison and self evaluation when the difference involves a status characteristic relevant to frequently occurring and continuing interaction. The likelihood of encountering someone whose occupational status, for example, is higher or lower than our own varies among societies, communities, neighborhoods, and work organizations. The frequency of social comparison theorem states: The more heterogeneous a person's social experience with respect to any status characteristic, the greater will be the person's self investment in that characteristic. The argument on which both these theorems is based involves a distinction between *knowledge* about one's location in a status hierarchy and *reminders* of that location and asserts that the latter is more consequential for self esteem. Most people know whether they are beautiful or ugly, wealthy or poor, and high or low occupational achievers, but, if my argument is correct, being ugly, poor, and a low occupational achiever is less likely to lower self esteem if one's social experience does not call attention to these characteristics.⁵

⁵ Who it is that reminds us of our location in any status hierarchy clearly affects the impingement of that reminder on our self esteem. Significant others are customarily identified as those who have most effect on how we feel about ourselves and are

STATUS-ASSIGNMENT SYSTEMS

If it is true that the effect of occupational status on our self esteem is determined by the frequency with which we are reminded of this status, then it becomes important to identify the conditions that make occupational status salient in social encounters. In this usage, salience means the frequency with which particular status characteristics are involved in social evaluation or social comparison. Analysis of what I am calling status-assignment systems focuses on the particular values used in the social settings in which status differences are actually experienced. The basic elements of a status-assignment system are a set of persons or positions being evaluated, a set of people doing the evaluating, a set of values, and a resulting hierarchy of persons or positions. Any social encounter that includes relations of deference, equality, or derogation involves an explicit or implied status-assignment system. Different systems may be invoked in the same interaction episode as the content of the interaction changes, but, whenever status differentiation occurs, some status-assignment system is involved. It represents the frame of reference in which a status level has meaning.

The term "system" is used here simply to indicate that status allocation occurs within ordered and bounded units. Deference entitlement involves some set of people who are compared and some who hold the values that form the bases for status claims. In practice, we can usually anticipate which specific others are likely to honor such claims. The clarity of the boundaries of status-assignment systems, that is, the ease with which we can determine who is in or out, is a variable attribute of such systems that, as we shall see, may have an effect on the salience of occupational status. The point here, however, is that social honor is allocated in more or less bounded units with structural properties that affect the link between the status-assignment and self esteem maintenance processes.

Occupational status-assignment systems may be more or less inclusive. They may be characterized by values unique to a particular workplace or shared by a total society. Occupationally based hierarchies may be limited to comparisons of positions in a particular firm or persons in a partic-

therefore, by *definition*, the people who evaluate us in terms of activities or attributes in which we are self invested. In order to avoid the tautology involved in the assertion that evaluation by significant others *produces* self investment, it is necessary to specify what makes others "significant," i.e., what makes their evaluation consequential for self esteem. Frequency of contact with others undoubtedly influences how often we are evaluated by them and how often we compare ourselves with them, and therefore this variable can be seen as one that contributes to the significance of others. Variables that affect either the preference or the necessity for continued frequent contact with other persons should also influence the extent to which these persons are significant others and would need to be taken into account in analyses of the effect of social experience on self esteem.

ular family or may include the full range of occupational types in a community or society. The collectivities in which status criteria and status hierarchies are embedded identify the persons most likely to use particular criteria in assigning status. If the collectivity is our family, we are usually able to anticipate with whose occupational achievement ours will be compared and how it will be defined. However, this is also generally true of a casual encounter with a stranger on an airplane, although the status system that is invoked in this instance is likely to be much more inclusive. When a stranger asks what we do for a living, we expect that his or her evaluation of our response will involve our occupation's general standing in the total society because the only value system in which strangers can assume mutual participation is at the broadest level of system reference. If it should turn out that we are in the same occupation as the stranger or employed by the same firm, a different and less inclusive status system becomes relevant.

An occupational status-assignment system may be circumscribed by the boundaries of a work group, an office, a firm, a job, an occupation, or a set of occupations in a community or society. It may involve interpositional comparisons of the kind involved in occupational prestige scales or intrapositional comparisons of work-role performance. The values on which comparisons are based may be widely held or situationally unique. Whatever their form, status-assignment systems are necessarily involved when status distinctions are made.

The structure of occupational status systems varies in ways that can be expected to influence the probability that work-related values will become relevant to social encounters. In the following, I will discuss some structural variables that may affect the salience of occupational status in the processes of social evaluation, social comparison, and, therefore, self assessment.

THE STRUCTURE OF STATUS SYSTEMS

Clarity of System Boundaries

The most appropriate structural variable with which to begin this discussion is the extent to which status systems are clearly bounded. Status-assignment systems exist only to the extent that it is possible to identify who is or is not a participant. Also, variation in the clarity of their demarcation is clearly related to other basic structural characteristics of status systems, such as the degree of consensus regarding status criteria and status placement. Thibaut and Kelley (1959, p. 229) use the term "status system" to refer to a group in which there is "general agreement, or consensus, as to the status of each group member." While some degree

of consensus and some clarity of system boundaries are required in order to speak of a status-assignment system, both can be seen as variable attributes of such systems.

Clarity of status-system boundaries, as it is used here, refers to several interrelated dimensions. One is the specificity of comparative reference groups. In some work settings, a new-car salesroom, for example, everyone knows exactly whose performances are being compared: sales records of all sales representatives in the firm are often prominently displayed on a wall of the showroom. Taxi driving represents an occupation in which there is less likely to be a specific set of others with whom work performance is compared. Taxi drivers may compare their driving skills with nonprofessional drivers, but this is an amorphous and heterogeneous reference group.

Another example of variation in the specificity of comparative reference groups can be seen in the difference between organizations that promote from within and those that include outside candidates. These procedures produce a difference in the clarity of status-system boundaries that is likely to affect the salience of particular evaluative criteria and also the importance of the promotion decision to self esteem. In companies with a policy of internal promotion, the criteria and standards for promotion decisions will be determined, in part, by the qualifications of the available candidates. Since these qualifications are apt to be recognized by each candidate, it is easier to assess the chance for promotion. This means locating oneself in a status hierarchy composed of one's colleagues that provides knowledge useful in deciding when to assert a status claim. Persons who recognize that there are older, more experienced, or better-qualified candidates for promotion are less likely to seek promotion actively and less likely to suffer loss of esteem if they do not receive it. For someone who believes he or she is the best-qualified candidate, the promotion decision represents a "test" of an evaluated self identity, resulting in some gain or loss in self esteem. This response is likely to be strengthened by the fact that successful or unsuccessful candidates can compare themselves, and be compared by others, with those persons with whom they regularly interact. If, however, a promotion policy includes consideration of outside candidates, it is apt to be less clear which criteria are being used, who is being compared with whom, and, if someone from outside the organization is selected, the threat to self esteem should be less, since all those who can regularly compare one another's previous occupational performances were equally unsuccessful.

A second dimension of the clarity of status-system boundaries is the degree of specificity regarding who can legitimately evaluate whom. Legitimacy as a status judge is most often determined by degree of competence, and any lack of clarity here is most likely to result from lack of

Status

consensus regarding the criteria for assessing competence. When the disagreement regarding status criteria is between those in an occupation and those outside it, the question of whose evaluations count is likely to be especially salient. The use of status criteria not shared by the general public tends to generate boundary-maintenance efforts that result in a high degree of consensus about the appropriateness of particular values and a clear specification of who can legitimately evaluate whom. There are some occupations in which the attempt to identify and maintain status-system boundaries is evident. These tend to be occupations with low interpositional status but frequent evaluation on intrapositional or work-role performance criteria that are accepted only in the occupation. Studies of these occupations suggest a degree of self investment in work higher than their prestige levels would predict. In many of them, this self investment is anchored in an intrapositional prestige hierarchy involving criteria that differ from those on which interpositional rankings are customarily based. These occupations also tend to be characterized by some measure of isolation produced by place of work, working hours, or other aspects of the job. Coal miners in isolated mining towns, for example, appear to have a love/hate orientation to their occupation in which the strenuous, physically unpleasant, and dangerous aspects of the job become sources of pride (Dennis, Henriques, and Slaughter 1956). The ability to withstand these hardships differentiates the coal miner from the outsider in terms of an uncommon set of occupational status criteria that, in addition, serve as a basis for status differentiation among the miners themselves. Freilich's (1958) description of occupational values among Iroquois structural-steel workers also suggests self evaluation in terms of courage displayed on the job. Working at great heights on these jobs affords opportunity for display of courage, and the amount of isolation produced by movement of work crews from one construction site to another helps insulate the structural-steel worker from evaluation by other, more commonly used status criteria. Unskilled construction workers in England, called "navvies," who live in camps at construction sites, also exhibit behavior indicating a rejection of the criteria on which their occupation is most commonly evaluated. Sykes describes this behavior as follows:

The roughness and danger of the work and the roughness of living conditions helped to give the navvies a low status. The navvies complained of this but they constantly boasted of their toughness in doing a hard and dangerous job and in working and living in conditions few men could stand up to. At work they did nothing to improve conditions and the younger ones would compete in doing the more dangerous jobs and in taking unnecessary risks. The men who did the most dangerous work cultivated a tremendous swagger, thus the men on tunneling liked to be known as

American Journal of Sociology

"Tunnel Tigers" and would wear the protective helmets that distinguished them from other men all the time. [Sykes 1969, pp. 161-62]

The use of unusual status criteria generally produces clearly bounded status systems in which there is a strong sense of in-group/out-group differentiation. Under these circumstances, the criteria that serve as the basis for group identification are apt to be very salient in social encounters among group members. While the number of people outside the occupation who are willing to honor status claims based on these criteria may be small, status differentiation involving the "deviant" criteria may occur frequently to the extent that interaction is restricted to the collectivity within which these values are shared.

Other things being equal, the greater the clarity of status-system boundaries, the more salient the status criteria involved. The extent to which occupational status systems are clearly bounded may affect the salience of work-related values in two ways. First, there may be direct effects in the sense that where it is clear who is to be compared with whom on which criteria, comparisons involving these criteria are more likely to be made. Also, where it is clear who can legitimately use which criteria in assigning status, social evaluations based on these criteria could be expected to be more common. A second way in which the clarity of status-system boundaries may influence the salience of occupational status is indirect, operating through its effect on other characteristics of status-assignment systems. We would expect, as noted above, a positive relationship between clarity of status-system boundaries and the degree of *consensus* in the system regarding both the status criteria to be used and the location of persons or positions in the status hierarchy. The extent of agreement regarding status criteria and status locations should influence the frequency with which the status criteria are invoked.

Consensus in Status-Assignment Systems

There is evidence that lack of consensus in others' evaluations of us and ambiguity in the criteria used in such evaluations may lead to unrealistically high self evaluations (Sherwood 1967; Felson 1981). One explanation is that consensus focuses attention on achievement level and constrains our ability to evaluate ourselves as we wish. In some occupations there is little ambiguity regarding either the bases for assessing occupational achievement or the location of persons and positions in intra- and interoccupational hierarchies. Most brain surgeons, for example, can be located in a clearly identifiable status hierarchy in the hospitals and communities in which they work and, to some extent, at national or international levels as well. The criteria on which these hierarchies are based are consensually held, and an operation can be seen as skillfully performed

whether the patient survives or not. Whatever their level of role performance, surgeons occupy an unambiguously prestigious position in hierarchies based on *interoccupational status*.

In contrast to brain surgery, social work is an example of an occupation in which its practitioners are more likely to experience difficulty in determining where they are located in either inter- or intrapositional status hierarchies. Formal organizational titles such as "department head" or civil service ratings may indicate status distinctions, but the criteria on which these distinctions are based are less clear than for the surgeon. One reason is that the "output" of the social worker's efforts is less easily compared with that of others. In some occupations, the output from work is highly visible, as in surgery, or has a common metric, as, for example, number of cars sold per month. In other occupations, such as social work or public school teaching, the performance of work is less often observed by colleagues, supervisors, or others deemed competent to evaluate it, and work-role performance is not ordinarily measured on any single yardstick. These conditions necessitate careful management of the accounts of work experiences given by social workers to colleagues and supervisors (Pithouse 1985).

There is also greater ambiguity regarding the location of social work in an *interoccupational status hierarchy*. The distribution of scores for occupations that, like social work, fall in the middle range on occupational prestige scales is often characterized by a greater dispersion around the mean, which reflects lack of consensus regarding the status they should be accorded. In the original NORC study of occupational prestige, status ratings of "welfare worker for a city government" ranged across all response possibilities from "excellent" to "poor," with the bulk of the responses divided approximately equally between "good" (43%) and "average" (35%). Ratings of "physician," in contrast, ranged only from "excellent" to "average," and 67% of the responses were in one category, "excellent," with most of the rest (30%) in the "good" category (Reiss 1961). Differences of this kind in the amount of agreement regarding the status of an occupation can be expected to produce variations in people's responses when they learn what someone else does for a living: physicians can expect uniformly positive evaluations, while social workers are likely to be uncertain about what to expect.

For the social worker with a high level of self investment in the occupational role, this uncertainty poses some obvious problems for self esteem maintenance. Where there are inconsistent evaluations by others, the process of seeking confirmation of status claims is necessarily a more active quest because of the self doubt these inconsistencies produce, and this process may induce more frequent evaluation of occupational achievement (Festinger 1954). That this condition may generate anxiety is

American Journal of Sociology

suggested by a study showing that ambiguity in promotion procedures causes stress among public school teachers (Bacharach, Bauer, and Conley 1986).

A consensually validated status hierarchy means that there is a clear and consensually held definition of achievement in situations where the values producing the status hierarchy are relevant. This condition is most likely where there is a high concern with achievement, and it will have a reciprocally reinforcing relationship with the salience of the relevant status characteristic: salience of a status characteristic induces pressure toward consensual validation of status differences, which in turn calls attention to the dimension or dimensions on which the status difference is based. There are circumstances, however, in which status may become problematic because its meaning is unclear. In the discussion of social work above, I have suggested that *ambiguity* regarding the definition of achievement may result in self doubt and an attempt to achieve confirmation of status claims that necessarily focuses the attention of others on occupational status. If both consensus and dissensus on status criteria can increase their salience, then we need to introduce some additional, mediating variables to explain this relationship.

One of these is the level of self investment in the activity to which the status criteria are relevant. Ambiguity regarding the meaning of achievement will produce concern about how others are evaluating us and result in self doubt only with regard to activities that have some consequence for our self esteem maintenance. A vague definition of achievement is, in fact, an advantage in activities where we have little or no self investment. The social worker in the example above would have no reason to induce work-related evaluations by others if he had low self investment in work. Inconsistency in others' evaluation is not a problem so long as the person is not seeking recognition for occupational achievement; in fact, a person who sees work simply as the source of a steady income is likely to *seek* an occupation in which status placement is ambiguous. This condition decreases not only the frequency of evaluation by others but also the personal responsibility for low achievement. We are less likely to be faulted or to fault ourselves for not "getting ahead" if there is no agreement about what this means. There is some evidence that the self-serving attributional bias is most likely to occur under conditions of high self investment (Miller 1976).⁶

⁶ Ambiguity regarding status criteria or status placement removes one constraint on our ability to attribute our failure to external causes. Although there is an extensive research literature on internal and external loci of control, few studies have dealt with the effect of work experience on these beliefs. There is some evidence, however, that occupational achievement increases the expectancy of internal control (Andrisani and Nestel 1976).

Self investment in work generates social situations in which the *objective* of interaction is to establish differences in occupational status, and, when this is true, dissensus regarding the criteria to be used in this process may increase the salience of occupational achievement. The affirmation or development of a status hierarchy is more easily accomplished when there is agreement regarding the criteria on which it is to be based. Status ambiguity or dissensus will prolong the process, thereby increasing the frequency and salience of the evaluative component of the interaction. Competition among professional bowlers or among salesmen may fairly easily resolve itself into an established status hierarchy, for example, whereas competition for status among sociologists will be prolonged by lack of agreement on the definition of occupational achievement.

The effort required to get favorable evaluations from others when status placement is ambiguous is one example of the link between consensus in occupational status-assignment systems and the salience of occupational status. Another example, already discussed, involves the use of "deviant" status criteria. The resulting boundary-maintenance effort not only increases consensus in the group regarding these values but also focuses evaluative attention on them. To the extent that an occupational attribute embodies a valued trait on the basis of which people see themselves as different from others, work is apt to be an especially salient status characteristic.

At the other extreme on this dimension are occupations in which the evaluative criteria are widely accepted and generally applicable to both work and non-work-related situations. These conditions may also increase the frequency with which people are reminded of their occupational status. For the reasons above, if status level is completely unproblematic, status differences may be taken for granted and not become the object of attention. If, however, an occupation has generally highly valued characteristics, its status is more likely to be noted. Persons whose occupations involve high levels of skill, responsibility, and autonomy may be made aware of this fact by the number of people, both at work and away from the workplace, who are willing to accord social honor on these bases. For this same reason, low occupational skill, responsibility, and autonomy may more often be a source of derogation. Occupational status appears more likely to be the object of attention for occupations at either extreme of occupational status hierarchies. Greater consensus regarding status criteria and status placement among these occupations may help to explain this.

Occupational status criteria vary in the number of different settings to which they are relevant. The bases on which persons in some occupations are evaluated are also applicable to many non-work-related tasks and

American Journal of Sociology

may serve as bases for status differentiation outside the workplace (Ross 1954). The exercise of responsibility, for example, is valued in a variety of non-work-related settings. If a person is assigned a responsible position in a community organization *because* he or she holds a responsible position at work, this experience may serve as a reminder of occupational achievement level. However, not all of the bases on which occupational status is assigned are equally generalizable. Within groups that differentiate occupational status on the basis of courage or of command of some esoteric knowledge, the situations off the job to which these criteria will be relevant may be limited. As an example, American literature professors may find occupationally based knowledge an advantage at dinner parties, whereas the shoptalk of proctologists is less likely to be of general interest. For our purposes, the important point in these examples is that people in some jobs are more often reminded of their occupational status by non-work-related experiences. Other things being equal, the more generalizable the occupational status criteria, the more frequent will be the occasions on which people are reminded of their occupational status.⁷

Heterogeneity in Status-Assignment Systems

Some persons are regularly confronted at work, at home, in their neighborhood, or in voluntary associations with others whose level of occupational achievement is higher or lower than their own. While, for other persons, this is uncommon. Because there is a broad range on this variable, and because it is difficult to ignore status differences in face-to-face interaction, frequency of contact with persons at different occupational status levels probably accounts for a large proportion of the variance in the salience of work-related values for status-assignment and self esteem maintenance processes. There is evidence that, the more distinctive one's traits or abilities in a social group, the more salient those traits or abilities will be in situational self definitions (McGuire and Padawer-Singer 1976). We expect that, the more heterogeneous in occupational status are the participants in any social encounter, the greater the probability that occu-

⁷ Turner (1978) makes a similar point regarding the effect of "overlapping of social circles" on the merger of role and person. Another example of generalizability of status criteria is the way in which they enter into "paths of task relevance" (Berger and Zelditch 1985). A path of task relevance is defined as "a status-task connection between the actor and the task that links the status characteristic possessed by the actor to an outcome state of the task, either success or failure." The salience of a status criterion can be expected to be greater to the extent (a) that it is involved in a larger number of paths of task relevance, (b) that the paths are short, and (c) that the links between the criterion and task outcomes are direct.

pational status will influence the content of the interaction in that encounter.⁸

Contact with persons who differ from us on any status characteristic may increase the salience of that characteristic in at least two ways. First, it can reasonably be expected to increase the frequency with which we compare ourselves with others in terms of the dimension on which we differ (Festinger 1954). If we are especially attentive to the ways in which we differ, and if the differences occur along a graduated parameter, then social comparisons necessarily entail self evaluations. Dimensions on which we are evaluating ourselves are very likely to enter into and influence the nature of our interaction with others.

A second way in which structural heterogeneity may influence the salience of a status characteristic is through its effect on evaluation by others. Ways in which we differ from others are likely to be noticed not only by us but also by other people, and that we are not "keeping up with the Joneses" is more likely to be called to our attention if we are seen with the Joneses in the same workplace, neighborhood, or church. Also, as noted earlier, contact with others having higher interoccupational status may call attention to and threaten the status of persons in occupations where unusual criteria are used for assigning status. On the basis of a study of social honor among meatcutters, Meara (1974, p. 279) concludes that, "when the source of a group's honor is exclusively within the group, members must work to protect this honor from disrespectful outsiders," and "therefore it is heteronomy rather than dirty work which poses the greatest threat to their honor."

Among the variables that affect frequency of contact among persons of unequal occupational status are the communication patterns of different jobs. Coal miners are called on, for the most part, to interact only with other coal miners, while receptionists, salesclerks, and others in organizational "boundary" positions are frequently in contact with persons whose occupational status differs from theirs. Patterns of interaction in non-work-related organizations also differ in ways that may affect frequency of cross-status contact. Differences in church doctrine and liturgy, for example, attract either more or less occupationally heterogeneous members, and, among active participants, this will produce either more or less frequent interaction among persons at different occupational status

⁸ An exception to this proposition should be noted. Where the occupational statuses of participants are equal and high and where all participants are aware of this fact, any reference to occupational status is mutually congratulatory and, therefore, more likely to occur. However, where occupational statuses are equal and low, there is little reason to call attention to this fact except in the context of role-distancing efforts in which occupation is being rejected as a basis for status assignment.

levels. As another example, variation among communities in the relation of occupational status to income should have an effect on the heterogeneity of neighborhoods with respect to occupational status. The higher the correlation between income and occupational status, the less contact we would expect between neighbors with different levels of occupational status.⁹ The shape of occupational status hierarchies is another structural variable that may influence the frequency of contact among persons at different status levels: this frequency should vary positively with both the extent of vertical differentiation in the hierarchy and the equality of distribution of persons among status levels. In work organizations with many status levels and many people at each level, the probability of encounters among persons with different levels of occupational achievement should be higher.

Shape of Status Hierarchies

The effect of the shape of occupational status hierarchies on frequency of cross-status contact is only one of many ways in which this variable may influence the salience of occupational status. Some occupational status hierarchies are tall, with a large disparity in social distance between top and bottom, while others are relatively flat, and this difference is characteristic of both interpositional and intrapositional status differentiation. In a status system based on interpositional comparisons, the shape of the hierarchy is largely a function of the division of labor, that is, of the way in which skill, responsibility, or other valued characteristics of work are distributed among occupations. How work is divided among positions also determines, to some extent, the shape of intraoccupational status hierarchies. For any activity or attribute to serve as a basis for status differentiation, there must be some range of difference in which comparisons can be made. There are jobs in which intrapositional status differentiation is uncommon because it is difficult to find any work-related values to serve as the basis for status assignment. The job of the unskilled worker on an assembly line, for example, provides almost no basis for evaluation of quality of work performance. Neither effort nor skill has any significant effect on quantity or quality of output, and there is little opportunity for display of such traits as strength, courage, independence, or endurance that serve as bases for evaluation in some other unskilled jobs. Differences in status exist among members of work groups on as-

⁹ Blau (1977) refers to this variable as the "intersection" of graduated parameters, with maximum intersection occurring when the relationship between two variables is orthogonal. Intersecting parameters promote heterogeneity and intergroup relations and, if my argument is correct, should as a consequence increase the salience of the status characteristic on which people differ.

sembly lines, of course, but these differences are generally based on non-work-related values. With respect to my major concern here, the hypothesis suggested is that the narrower the range of quality in work-role performance, the less salient occupational status will be. A word of caution is necessary. What appear to an outsider to be minor differences in performance level may assume major proportions as status differentiators to those on the job. The fact remains, however, that there is a substantial difference among occupations in the potential for intrapropositional status differentiation, and, other things being equal, we would expect this difference to be associated with variation in the salience of occupational status.

It is necessary to distinguish the shape of status hierarchies as they may appear to outside observers and as they appear to the participants in a particular status-assignment system. Automobile workers, for example, are usually seen by social scientists, and may occasionally see themselves, as occupying a low-status position in a hierarchy that includes all positions in the firm. Studies of industrial workers have often assumed that their location near the bottom of the organizational hierarchy poses a threat to self esteem (Braverman 1974). The concept of multiple status-assignment systems allows for the possibility that automobile workers are located in different occupational status hierarchies depending on the persons with whom they interact and on the content of the interaction. The shape of the status hierarchy will differ depending on which status-assignment system is relevant to the social situation. There may be occasions when the automobile worker is assigned a status location in the tall hierarchy in the total organization, but much of the time, the only status differences relevant to social experience on the job occur in a flat hierarchy extending only from first-level supervisors down to custodians. Microecological characteristics of automobile factories and the authority and communication patterns in these firms tend to isolate workers from anyone above the first-level supervisor. Contact with a factory or corporate executive may invoke the more inclusive status-assignment system, but these contacts are uncommon. The flat inter- and intraoccupational status hierarchies that automobile workers are most likely to experience both on and off the job should decrease the salience of work-related status and reduce the extent to which workers are disadvantaged when their occupational status does become relevant to social interaction.

Various studies have shown that adjacent status levels are especially salient in status comparisons (Homans 1974). One explanation may be the greater probability of cross-status contact among persons whose status is only slightly different. The existence of small status differences does not, however, *necessarily* mean that these differences are frequently the object of attention, and it seems reasonable to expect that they will more likely

serve as reminders of status location if they are part of tall hierarchies that focus attention on status differentiation. Another explanation for the significance of adjacent status levels is that they may represent the next step in a mobility sequence. The shape of status hierarchies clearly influences the likelihood of occupational mobility.

Opportunity for Mobility

The shape of a status hierarchy is determined not only by the number of status levels but also by the social distances between two levels. In the army there are many status-differentiated levels, with some—second and first lieutenant, for example—having small gaps in social honor between them, while other adjacent ranks—colonel and brigadier general, for example—involve a large gap. The social distance between adjacent ranks can be expected to influence the salience of occupational status as a consequence of its effect on occupational mobility: A large number of status levels with small differences between them should increase both the expectation and the actual frequency of upward mobility. Clearly defined career sequences (e.g., apprentice/j journeyman/master craftsman or second vice-president/first vice-president/president) have the effect of producing the expectation of upward mobility. Because promotions generally become the focus of attention, movement through a status-ordered career sequence should increase the salience of occupational status. For those in such a sequence who fail to move up, the expectation held by co-workers, friends, and relations that they *should* be upwardly mobile is likely to produce an increasing emphasis on their location in the occupational status hierarchy. However, if a person is at the top of one sequence, and there is a large gap before the beginning of another, or if the job is not part of any career sequence, as is true of many low-status jobs, then further occupational achievement is less likely to be expected, and there is less onus on the failure to be upwardly mobile. In general, the greater the opportunity for upward occupational mobility, the greater will be the frequency of evaluation by others in terms of occupational achievement level. The well-known finding of *The American Soldier* (Stouffer et al. 1949) that dissatisfaction with promotion results from relative deprivation may also reflect that where promotion is more frequent, occupational status level will be more salient.

Another reason for the influence of the structure of occupational opportunity on the salience of occupational achievement is that it is partly responsible for whether success or failure is attributed to the individual. As noted above, we are less likely to blame individuals for low occupational achievement if their chance for upward mobility is limited. Where mobility channels are closed, we are also less likely to defer to those in

high-status positions since we will less often attribute their status location to their personal qualities or actions. Even when social evaluation or social comparisons do occur under conditions of limited opportunity for mobility, they are less likely to lead to internal attribution of the cause for success or failure and, therefore, should have less effect on self investment in work (Weiner 1986).

SUMMARY

There are other characteristics of occupational status-assignment systems that could be expected to influence the salience of occupational status, but the ones that have been discussed may be sufficient to illustrate my major thesis here. There is wide variation in the frequency with which people are made aware of their occupational status, and it is at least partly attributable to differences in the structure of occupational status-assignment systems. A status-assignment system consists of a set of persons or positions that are evaluated, a set of people who evaluate, a set of criteria used in status allocation, and a resulting status hierarchy. Status-assignment systems provide a cognitive frame for identifying appropriate comparative reference groups, legitimate status judges, and the level of success or failure. Whenever status distinctions are made in social situations, some sort of status-assignment system is involved. Among the structural characteristics of occupational status-assignment systems that can be expected to influence the salience of occupational status are the clarity of system boundaries, degree of consensus regarding criteria and status placement, commensurability of work output, inclusiveness of the status-assignment system, generalizability of status criteria, range of difference in work-role performance, frequency of contact with persons at different status levels, shape of status hierarchies, and opportunity for mobility.

Most studies of occupational status limit their focus to a national interoccupational prestige hierarchy and make the assumption that a person's location in this hierarchy is broadly relevant to his or her social experience. The consequences of occupational status differences with which these studies are most often concerned rest on the assumption that people necessarily care about these differences. In contrast, I have argued that there are many different occupational status hierarchies that may be used to define level of occupational achievement and that particular definitions of occupational status are embedded in recurring social relations. Occupational status differences, however defined, are seldom experienced by some persons, whereas they are the central feature in the social relations of others. While this line of argument requires that we attend to the meaning of occupational status as it is actually experienced, it does

American Journal of Sociology

not imply that studies in this area must be limited to Blumerian analyses of emergent meaning in specific social situations. Understandings from structural sociology should prove useful in studies of those features of status-assignment systems that influence the meaning and salience of occupational status. The argument presented here does require rejection of the assumption that work necessarily affects self esteem and directs attention to variables that may influence this relationship.

REFERENCES

Andrisani, Paul J., and Gilbert Nestel. 1976. "Internal-External Control as Contributor to and Outcome of Work Experience." *Journal of Applied Psychology* 61:156-65.

Bacharach, Samuel B., Scott C. Bauer, and Sharon Conley. 1986. "Organizational Analysis of Stress: The Case of Elementary and Secondary Schools." *Work and Occupations* 13:7-32.

Berger, Joseph, and Morris Zelditch, Jr. 1985. *Status Reward: and Influence*. San Francisco: Jossey-Bass.

Blau, Peter M. 1977. *Inequality and Heterogeneity*. New York: Free Press.

Bradley, Gifford Weary. 1978. "Self-serving Biases in the Attribution Process." *Journal of Personality and Social Psychology* 36:56-71.

Braverman, Harry. 1974. *Labor and Monopoly Capital*. New York: Monthly Review.

Burke, Peter J. 1980. "The Self: Measurement Requirements from an Interactionist Perspective." *Social Psychology Quarterly* 43:18-29.

Dennis, N., F. Henriques, and C. Slaughter. 1956. *Coal Is Our Life*. London: Eyre & Spottiswoode.

Dubin, Robert, R. Alan Hedley, and Thomas C. Taveggia. 1976. "Attachment to Work." Pp. 281-340 in *Handbook of Work, Organization, and Society*, edited by Robert Dubin. Chicago: Rand McNally.

Faunce, William A. 1982. "The Relation of Status to Self Esteem: Chain Saw Sociology at the Cutting Edge." *Sociological Focus* 15:163-78.

—. 1984. "School Achievement, Social Status, and Self Esteem." *Social Psychology Quarterly* 47:3-14.

Faunce, William A., and Robert Dubin. 1975. "Individual Investment in Working and Living." Pp. 299-316 in *The Quality of Working Life*. Vol. 1: *Problems, Prospects and the State of the Art*. Edited by Louis E. Davis and Albert B. Cherns. New York: Free Press.

Felson, Richard B. 1981. "Ambiguity and Bias in the Self-Concept." *Social Psychology Quarterly* 44:64-69.

Festinger, Leon. 1954. "A Theory of Social Comparison Processes." *Human Relations* 7:117-40.

Freilich, Morris. 1958. "Cultural Persistence among the Modern Iroquois." *Anthropos* 53:473-83.

Granovetter, Mark. 1985. "Economic Action and Social Structure: The Problem of Embeddedness." *American Journal of Sociology* 91:481-510.

Heider, Fritz. 1958. *The Psychology of Interpersonal Relations*. New York: Wiley.

Hoelter, Jon W. 1983. "The Effects of Role Evaluation and Commitment on Identity Salience." *Social Psychology Quarterly* 46:140-47.

—. 1986. "The Relationship between Specific and Global Evaluations of Self: A Comparison of Several Models." *Social Psychology Quarterly* 49:129-41.

Homans, George C. 1974. *Social Behavior: Its Elementary Forms*. New York: Harcourt Brace Jovanovich.

Status

Hughes, Everett Cherrington. 1958. *Men and Their Work*. Glencoe, Ill.: Free Press.

Kahl, Joseph A. 1965. "Some Measurements of Achievement Orientation." *American Journal of Sociology* 70:669-81.

Kaplan, Howard B. 1971. "Social Class and Self Derogation: A Conditional Relationship." *Sociometry* 34:41-64.

Kohn, Melvin L., and Carmi Schooler. 1983. *Work and Personality*. Norwood, N.J.: Ablex.

Marsh, Herbert. 1986. "Global Self Esteem: Its Relation to Specific Facets of Self-Concept and Their Importance." *Journal of Personality and Social Psychology* 51:1224-36.

Maurer, John G., D. J. Vredenburgh, and R. C. Smith. 1981. "An Examination of the Central Life Interest Scale." *Academy of Management Journal* 24:174-82.

McCall, George J., and J. L. Simmons. 1978. *Identities and Interactions*. New York: Free Press.

McGuire, William J., and Alice Padawer-Singer. 1976. "Trait Salience in the Spontaneous Self-Concept." *Journal of Personality and Social Psychology* 33:743-54.

Meara, Hannah. 1974. "Honor in Dirty Work: The Case of American Meat Cutters and Turkish Butchers." *Sociology of Occupations and Professions* 1:259-83.

Miller, Dale T. 1976. "Ego Involvement and Attributions for Success and Failure." *Journal of Personality and Social Psychology* 34:901-6.

Miller, Dale T., and M. Ross. 1975. "Self-serving Biases in the Attribution of Causality." *Psychological Bulletin* 82:213-25.

Mortimer, Jeylan T., and Michael D. Finch. 1986. "The Development of Self-Esteem in the Early Work Career." *Work and Occupations* 13:217-39.

Mortimer, Jeylan T., and Jon Lorence. 1979. "Occupational Experience and the Self-Concept: A Longitudinal Study." *Social Psychology Quarterly* 42:307-23.

Pithouse, Andrew. 1985. "Poor Visibility: Case Talk and Collegial Assessment in a Social Work Office." *Work and Occupations* 12:77-89.

Rabinowitz, Samuel, and Douglas T. Hall. 1977. "Organizational Research on Job Involvement." *Psychological Bulletin* 84:265-88.

Reiss, Albert J., Jr. 1961. *Occupations and Social Status*. New York: Free Press.

Rosenberg, Morris. 1965. *Society and the Adolescent Self-Image*. Princeton: Princeton University Press.

—. 1967. "Psychological Selectivity in Self-Esteem Formation." Pp. 26-50 in *Attitude, Ego Involvement and Change*, edited by Carolyn W. Sherif and Muzafer Sherif. New York: Wiley.

—. 1979. *Conceiving the Self*. New York: Basic.

Rosenberg, Morris, and Leonard L. Pearlman. 1978. "Social Class and Self-Esteem among Children and Adults." *American Journal of Sociology* 84:53-77.

Ross, Aileen. 1954. "Philanthropic Activity and the Business Career." *Social Forces* 32:274-80.

Schrauger, J. Sidney. 1975. "Responses to Evaluation as a Function of Initial Self-Perceptions." *Psychological Bulletin* 82:581-96.

Sherwood, John J. 1967. "Increased Self-Evaluation as a Function of Ambiguous Evaluations by Referent Others." *Sociometry* 30:404-9.

Stouffer, Samuel A., et al. 1949. *The American Soldier: Adjustment during Army Life*. Princeton: Princeton University Press.

Stryker, Sheldon. 1968. "Identity Salience and Role Performance: The Relevance of Symbolic Interactionist Theory for Family Research." *Journal of Marriage and the Family* 30:558-64.

—. 1980. *Symbolic Interactionism: A Social Structural Version*. Menlo Park, Calif.: Benjamin/Cummings.

Stryker, Sheldon, and Anne Statham Macke. 1978. "Status Inconsistency and Role Conflict." *Annual Review of Sociology* 4:57-90.

American Journal of Sociology

Stryker, Sheldon, and Richard T. Serpe. 1982. "Commitment, Identity Salience, and Role Behavior." Pp. 199–218 in *Personality, Roles, and Social Behavior*, edited by W. Ickes and E. Knowles. New York: Springer.

Swann, William B., and Stephen J. Read. 1981. "Self-Verification Processes: How We Sustain Our Self-Conceptions." *Journal of Experimental Social Psychology* 17:351–72.

Sykes, A. J. M. 1969. "Navvies: Their Social Relations." *Sociology* 3:157–72.

Tesser, Abraham. 1986. "Some Effects of Self-Evaluation Maintenance in Cognition and Action." Pp. 435–64 in *Handbook of Motivation and Cognition*, edited by Richard M. Sorrentino and E. Tory Higgins. New York: Guilford.

Tetlock, Philip E., and Ariel Levi. 1982. "Attribution Bias: On the Inconclusiveness of the Cognitions-Motivation Debate." *Journal of Experimental Social Psychology* 18:68–88.

Thibaut, John W., and Harold H. Kelley. 1959. *The Social Psychology of Groups*. New York: Wiley.

Turner, Ralph H. 1978. "The Role and the Person." *American Journal of Sociology* 84:1–23.

Vroom, Victor H. 1962. "Ego Involvement, Job Satisfaction, and Job Performance." *Personnel Psychology* 15:159–77.

Weiner, Bernard. 1986. *An Attributional Theory of Motivation and Emotions*. New York: Springer.

Wilensky, Harold. 1966. "Work as a Social Problem." Pp. 117–65 in *Social Problems: A Modern Approach*, edited by Howard S. Becker. New York: Wiley.

Wylie, Ruth C. 1979. *The Self-Concept*. Vol. 2: *Theory and Research on Selected Topics*. Lincoln: University of Nebraska Press.

Zetterberg, Hans L. 1966. "On Motivation." Pp. 124–41 in *Sociological Theories in Progress*, edited by Joseph Berger, Morris Zelditch, Jr., and Eo Anderson. Boston: Houghton Mifflin.

Similarity of Political Behavior among Large American Corporations¹

Mark S. Mizruchi
Columbia University

Political sociologists have debated for decades, without resolution, whether elites in advanced capitalist societies are integrated. Rather than ask whether elites are integrated, this study examines the conditions under which convergence of political behavior occurs, focusing on campaign contributions of political action committees in the American business community. A model of similarity in corporate political behavior is proposed that draws on principles developed by resource-dependence and social class theorists of intercorporate relations. The model was supported by an examination of the 1,596 dyads created by relations among 57 large U.S. manufacturing firms in 1980. Membership in the same primary industry or several similar industries, geographical proximity of headquarters locations (but not plant locations), market constraint, and common relations with financial institutions (through either stock ownership or directorate ties) were positively associated with the similarity of political behavior between firms. Market constraint affected the similarity of political behavior primarily because it increased the likelihood that firms would produce in the same industries. The effect of indirect board interlocking through financial institutions was a stronger predictor of similarity of political behavior than was direct interlocking between manufacturing firms. The findings suggest the simultaneous importance of organizational and social network factors in understanding common political behavior between firms.

For decades, social scientists have debated the extent to which elites in advanced capitalist societies are integrated. Pluralists have argued that

¹ This research was supported by two grants from the National Science Foundation (SES-8619230 and Presidential Young Investigator Award SES-8858669) and by the Department of Sociology at Columbia University. Earlier versions of the paper were presented at the annual meetings of the American Sociological Association, Chicago, August 1987, and the Nags Head Conference on Corporate Interlocks, Kill Devil Hills, North Carolina, September 1987. I would like to thank Henry Mayorga for his assistance with the data collection and Bernard Barber, Peter Blau, Ron Burt, Paul DiMaggio, Miguel Guilarte, David Jacobs, Harrison White, and Mayer Zald, none of whom is responsible for what follows, for comments. Requests for reprints should be sent to Mark S. Mizruchi, Department of Sociology, Columbia University, New York, New York 10027.

American Journal of Sociology

elites in Western societies were becoming increasingly atomized, or, as Dahrendorf (1959, p. 47) put it, a series of "partly agreed, partly competing, or partly simply different groups." In rebuttal, elite theorists have argued that despite disagreements over specific issues, elites have remained highly integrated. Marxists too have debated the extent to which the business community was capable of unified political behavior (see Carnoy 1984; Alford and Friedland 1985, for useful reviews of these debates).

After many years of these disputes and little progress toward their resolution, one fact has become abundantly clear: elites in advanced capitalist societies cannot be said in the abstract to be either unified or fragmented. There are times in which elites act in a unified manner and times in which they do not. What is needed, therefore, is a study of the *conditions* under which elites act in a unified manner; in other words, the factors that determine whether elites will cohere on a particular issue or series of issues.

This article examines the sources of political convergence in the business community, drawing on concepts developed in organizational and political sociology.

BACKGROUND

The issue of elite unity cuts to the heart of debates about the extent to which advanced capitalist societies are democratic. As far back as the 1940s, Schumpeter (1942) conceded that most political activities in industrialized Western societies were dominated by elites. What enabled these societies to remain democratic, according to Schumpeter and later pluralists, was the prevalence of conflict among elites. These theorists argued that, even if most citizens are not actively engaged in politics, they have choices between candidates for office and they have the opportunity to participate in politics.

Early critics of pluralism, beginning with Mills (1956), argued that the conflicts that existed among elites were insufficient to outweigh the essential unity of interest that bound them together. However, despite several suggestive hypotheses, Mills was unable to demonstrate the existence of elite political unity. And in recent years, most theorists, even those who posit elite unity, recognize that there are strong tendencies toward divergence in the business world. What currently distinguishes unity, or class, theorists from disunity, or pluralist, theorists is the formers' view that mechanisms exist that are capable of mediating and resolving intercorporate disputes (Pfeffer [1987] presents a cogent discussion of this distinction).

Corporations

Despite considerable theorizing about the role of various factors in business conflict resolution, very little is known about the conditions under which successful resolution takes place. Although Whitt (1982) found that business was unified in all five transportation issues that he examined, Bauer, Pool, and Dexter (1972), in a study of protective tariff legislation, failed to find evidence of business consensus. Despite the obvious value of detailed case studies, the divergent findings of these two studies illustrate the dangers of generalizing from them. Quantitative studies in this area, however, have tended to focus on structures of interlocking directorates or stock ownership relations without examining the political consequences of those relations. A major reason for this neglect has been the absence of appropriate data on corporate political behavior.

In recent years, however, as data on campaign contributions of political action committees (PACs) have become publicly available, several social scientists have begun to employ PAC data to examine business political behavior.² Political scientists and economists have produced a huge literature on corporate PAC contributions (see Mizruchi 1989a for citations). Sociologists who have employed PACs as data have been less concerned with detailed analysis of PACs per se and more concerned with employing PAC data to examine theories about the extent of integration in the business community and the effects of corporate network structures on business political activity (Koenig and Gogel 1981; Etzioni 1984; Kouzi and Ratcliff 1985; Clawson, Neustadtl, and Bearden 1986; Burris 1987). These studies have greatly increased our knowledge of the behavior of political action committees. But far more work needs to be done to gain an understanding of the conditions under which business engages in unified political behavior.

MEASURING INTEGRATION IN THE BUSINESS COMMUNITY

The difficulty of studying the determinants of political integration in the business community is compounded by the fact that the variable has been conceptualized in several different ways. Cohesion, which along with unity is the most commonly used term, has been viewed primarily in

² In accordance with the Federal Election Campaign Act of 1971, all corporate contributions to political campaigns must be filed with the Federal Election Commission, and information about all such contributions is available to the public. For detailed discussions of the nature of PACs, see Handler and Mulkern (1982) and Sabato (1984). Laumann and his colleagues have produced several quantitative studies of political behavior, but their focus has included nonprofit as well as profit-making organizations. See, e.g., Laumann, Marsden, and Galaskiewicz (1977) and Laumann and Knoke (1987).

American Journal of Sociology

terms of either the commonality of objective interests (as in Marx's concept of "class in itself") or the existence of a common group consciousness as in Marx's "class for itself"). Most uses of cohesion assume the existence of a certain level of group interaction.³

One possible manifestation of cohesion, which is not necessarily dependent on direct interaction but which is important in its own right, is the extent to which different corporations exhibit similar political behavior. Such behavior could consist of common support for legislation, government policies, or the candidacies of particular individuals. The focus on behavior is important for two reasons. First, as Dahl (1961) has pointed out, if common attitudes or interests are not translated into unified political activity, then even an otherwise unified elite will have little effect on the political process. Second, it is often difficult to decipher the precise motives behind political positions taken by business. Firms may engage in similar behavior for many reasons. They may share economic interests. Their leaders may share political values or attitudes. One firm may go along with the actions of another because it is subject to various degrees of overt or covert pressure.⁴

In some cases, even firms with otherwise conflicting interests may engage in similar behavior. For example, two firms with opposing positions on tax laws may both contribute to the candidacy of House Ways and Means Chairman Dan Rostenkowski in the hope of influencing him. It is crucial to point out, however, that regardless of whether the two firms have shared or opposing positions, the outcome of the two decisions (common support for Rostenkowski) operates as if they shared the same position. That is, the consequences of common political behavior are the same regardless of whether the firms share the same interests (Merton 1936).⁵

Corporate unity has generally been perceived as a characteristic of a particular national or local business community at a particular time.

³ See Mizruchi (1989b) for an extensive discussion of this issue.

⁴ As Williams (1970, p. 583) points out, "There is a wide range within which an important degree of cohesion, in the sense of coordinated activity, can be maintained by coercion, the effective threat of the few over the many. . . . Not all cohesion rests on consensus or voluntary participation." For a classic discussion of the conditions under which internal conflict facilitates cohesion, see Coser (1956).

⁵ Most of the literature on PACs suggests that common support for the same candidates is generally indicative of shared rather than conflicting interests (Sabato 1984, pp. 44-49). Reasons for this include the fact that members of particular industries, which generally share political interests even though they compete economically, tend to have similar contribution patterns (virtually all studies find that industry is a significant predictor of contribution patterns), and that businesses, even those that otherwise pursue widely different political strategies, rarely come into direct opposition in particular races (Clawson et al. 1986).

Corporations

However, without a comparative or historical focus, the concept has limited meaning. Data on PACs have not been available for enough years to undertake time-series analysis on characteristics of the corporate community as a whole. To ensure sufficient degrees of freedom, I shall employ dyads, relations between pairs of firms in a given year, as the units of analysis.

The use of dyads to measure integration does narrow the focus of the study from the characteristics of the system as a whole to the characteristics of relations between individual units in the system. It is difficult, of course, to generalize about system-wide characteristics on the basis of the characteristics of dyads. Dyadic relations, however, do form the building blocks of larger network structures (Laumann and Marsden 1982). Although it would be ideal to have data on the degree of political similarity in the entire corporate community over enough time periods to conduct a time-series analysis, an examination of dyadic relations is a useful first step.

THE DEPENDENT VARIABLE

The dependent variable in this study is the similarity of political behavior of pairs of firms, defined as the extent to which the two firms contributed to the same congressional candidates in the 1979–80 election cycle. Clearly, there are other aspects to corporate political behavior than a firm's contributions to electoral campaigns. Nevertheless, the overwhelming consensus among PAC researchers is that regardless of the effect of PACs on the outcome of elections or on the voting behavior of individual legislators, corporate CEOs view PAC contributions as an important aspect of the firm's overall political strategy (Handler and Mulkern 1982, pp. 61, 72–78; Sabato 1984, pp. 34–35, 49; Useem 1984, pp. 139–41). I have presented (Mizruchi 1989a, app. A) a detailed discussion of the meaning and usefulness of common political contributions as a variable. One point, however, deserves mention here.

Many sociologists and political scientists believe that corporate PACs frequently contribute to two or more candidates in an election in order to hedge their bets. If this were true, an analysis based on common contributions between pairs of firms would be misleading since common support for one candidate would ignore the existence of one or both firms' simultaneous support for the candidate's opponent. In fact, however, such "split giving" is rare and is frowned upon in the PAC community (Handler and Mulkern 1982, p. 84; Rothenberg and Roldan 1983, p. 10; Sabato 1984, p. 88). In an earlier study (Mizruchi and Koenig 1986), it was found that common gifts to two or more candidates in an election occurred in fewer than 2% of all contributions. Moreover, many cases of

American Journal of Sociology

split giving involve contributions to a losing candidate in a primary, followed by a contribution to a different candidate in the general election (Handler and Mulkern 1982, p. 85). The issue of split giving was handled in the earlier study by removing all cases of split giving from the data file. This modification made virtually no difference in the results. Therefore, there was no need to repeat the procedure here.

THE ROLE OF ECONOMIC INTERDEPENDENCE

Although several theorists have identified sources of convergence in the business community, one factor that has received little explicit attention is the role of economic interdependence. Some theorists have hinted at economic sources of cohesion (Baran and Sweezy 1966; Herman 1981; Whitt 1982; Useem 1984; Mintz and Schwartz 1985; Jacobs 1988), but no one has explicitly built economic interdependence into his or her model. The role of interdependence in business unity is particularly important because it is an issue on which two major competing perspectives on intercorporate relations, the resource-dependence and social class models, converge (Mizruchi 1987; see also Zald 1970; Pfeffer 1987). The model presented below draws on both perspectives.

Theoretical arguments do exist to support the notion that economic interdependence facilitates political cohesion. In *The Division of Labor in Society*, Durkheim argued that interdependence was the basis of social order in modern societies. In more recent formulations, several scholars have argued that the dependence of one actor on another gives the latter power over the former (Emerson 1962; Blau 1964). Implicit in these views is the notion that power can be used to maintain solidarity under conditions favorable to the powerful actor even if the two actors have otherwise conflicting interests.⁶

Earlier research on this topic (Mizruchi and Koenig 1986) suggested the importance of distinguishing interdependence defined as the volume of transactions between firms from dependence defined by the level of economic leverage that one firm exerts over another. Burt (1983) has developed the concept of "constraint" to account for the extent to which dependence is affected by the absence of alternatives. An industry that does a lot of business with a relatively competitive industry will be subject to less constraint on its autonomy than an industry that does the same amount of business with a relatively concentrated industry. Burt has employed this

⁶ In a small-group experiment, Cook and Emerson (1978) found that powerful actors were able to use their power to maintain solidarity in exchange networks.

Corporations

model to demonstrate that more concentrated industries maintain higher profit margins.

For corporate political behavior, it is logical to assume that the existence of constraint in an interdependence relation facilitates unity of political behavior.⁷ Whitt (1982), for example, has described a case in which several San Francisco banks that had originally supported a statewide mass-transit proposal reversed their position. This appeared to be due to the anticipation of pressure from oil companies on which the banks were dependent for business. Findings from an earlier study (Mizruchi and Koenig 1986) support this notion. In an analysis of 91 dyads created by relations between the two largest members of 14 major two-digit industries, Mizruchi and Koenig found that the level of constraint between industries was positively related to the similarity of their political behavior.⁸

The concept of interdependence implies a reciprocal dependence of two actors on each other. The concept of constraint, on the other hand, implies that one actor constrains the other. Given the principles of the power-dependence theorists cited above, we might expect similar behavior to occur primarily in cases in which constraint is a one-way process. Yet interindustry constraint relations tend to be relatively symmetric, and I found, in an earlier study, that the level of symmetry in the constraint relation had no effect on either the similarity of political behavior or the magnitude of the constraint coefficient. This finding appears contradictory to the power-dependence model, but it is not (see, e.g., Emerson 1962, pp. 33-34). When members of two concentrated industries are highly dependent on each other, neither has a large number of alterna-

⁷ This is consistent with the argument by DiMaggio and Powell (1983, p. 154) on the sources of isomorphism of organizational structures (see their hypotheses A-1 and A-2).

⁸ The results of that study suggested that, when one controls for constraint, the volume of transactions between industries will be negatively related to the similarity of political behavior. Two problems associated with this finding have led me to delete this variable from the present study. First, although there are several examples of interdependent industries that might have opposing political interests, there are many other examples in which interdependent industries have similar interests (e.g., American automakers and auto parts suppliers would probably both support import restrictions on foreign cars) or in which industries that transact little or no business (e.g., chemicals and tourism) might have opposing interests. Second, results from the present study raise the possibility that the strong, opposite effects of constraint and volume of transactions in the earlier study were due to collinearity (the variables in that study had a simple correlation of .65). We had doubted that collinearity was a problem because the simple correlations and the betas were of the same sign. But in the current study I found that volume of transactions was a positive predictor of similarity. Given its theoretical ambiguity and its confounding with constraint (which is the primary variable of interest), I have omitted this variable from the analysis.

American Journal of Sociology

tives, and both have a stake in maintaining a smooth relationship. Therefore, we can expect to find a positive association between constraint and behavioral similarity regardless of whether the constraint is mutual or one-way. The above discussion suggests the following hypotheses:

- H1 The greater the level of constraint in an interfirm relation is, the greater the similarity of political behavior.
- H2 The asymmetry of constraint between two organizations has no effect on the similarity of political behavior between them.
- H3 The asymmetry of constraint between two organizations has no effect on the strength of the relations in H1.

Because firms in the same industries are likely to be in equivalent positions with respect to resource dependence on other industries and because government policies frequently affect entire industries, members of particular industries should exhibit high levels of behavioral similarity. Therefore:

- H4 Organizations in the same primary industry will exhibit greater similarity of political behavior than organizations in different primary industries.
- H5 The more common the industries in which two firms produce are, when we control for their level of diversification, the greater the similarity of political behavior between them.

OTHER SOURCES OF UNITY

Class theorists have assumed that interlocking corporate directorates mediate intercorporate conflicts. To examine this hypothesis, I shall examine both direct and indirect interlocks. Mintz and Schwartz (1985) have argued that major commercial banks play an important role in corporate conflict resolution. Moreover, another study (Mizruchi and Koenig 1988) found a strong positive association between indirect interlocks through major banks and the similarity of corporate political behavior. Therefore, I shall examine the number of cases in which firms share directors with the same bank, as well as with one another.

Common stock ownership by financial institutions in two companies is another potential source of corporate political unity. Earlier results (Mizruchi and Koenig 1988) also revealed a positive association between the number of banks that held stock in members of a particular industry and the similarity of political behavior in that industry.

Most of the largest firms contribute to scores of candidates, from all geographical regions. Clawson et al. (1986), for example, found an aver-

Corporations

age of more than 80 contributions per PAC among the 243 firms in their study. The 28 firms in my earlier study contributed to an average of over 100 candidates each. Two previous studies (Mizruchi and Koenig 1986, 1988) revealed no effect of geographical proximity on the similarity of campaign contributions. Given its potential importance, however, geographical proximity should be included in any analysis of similarity of contribution patterns, even if only as a control variable. I shall therefore examine both the similarity of headquarters locations (the focus of previous studies) and the similarity of plant locations. This discussion suggests the following hypotheses:

- H6 The number of interlocking directorates, both direct and indirect, between two firms is directly related to the similarity of their political behavior.
- H7 The number of financial institutions that hold stock in two firms is directly related to the similarity of political behavior between those firms.
- H8 The greater the geographical proximity between two firms in terms of corporate headquarters and plant locations is, the greater their similarity of political behavior.

DATA AND MEASURES

Because I am interested in the political behavior of the largest American corporations, my universe for sampling is the *Fortune* 500 industrial firms (*Fortune* 1981). This study focuses on manufacturing corporations because of the comparability of data in these industries. The sample is designed to be representative of the widest possible range of industries in the 500. Therefore, I selected, for each of the 20 major manufacturing industries, the three largest firms whose primary operations were in those industries, according to *Fortune*, in 1980. For the few cases in which one of the three largest firms did not have a PAC in 1980, I included the next largest firm that had one. Because a small number of the 20 industries did not have three firms among the 500, the final sample consisted of 57 firms, which yielded a sample of 1,596 interfirrm dyads.

The data set for the dependent variable was the list of all contributions to congressional candidates in the 1980 elections. To measure behavioral similarity, I compared lists of every contribution made by each firm among all 1,596 pairs. The following measure of association was employed:

$$S_{ij} = n_{ij}/(n_i * n_j)^{1/2},$$

where S_{ij} equals similarity, n_{ij} equals the number of contributions in common, and n_i and n_j equal the number of contributions made by firms i

and j , respectively. This measure, which is identical to the measure of association employed in several studies of interlocking directorates (Mariolis 1975; Mizruchi 1982), provides an indicator of behavioral similarity that is independent of the number of contributions by each firm.⁹

For each firm, I recorded the two-digit industry in which the firm did the most business and all the two-digit industries in which it produced. For each dyad, I coded a dummy variable indicating whether the two firms had primary involvement in the same industry, as well as the number of two-digit industries in which they were both involved. The latter variable was standardized by dividing the number of common industries by the square root of the product of the number of industries in which each firm produced (the same measure of association employed to measure the dependent variable).

Constraint is defined by Burt (1983, pp. 36–60) as the dependence of one industry on another for sales and purchases, weighted by the concentration of the latter industry. Based on hypotheses H1–H3, I have operationalized three variables relevant to constraint: the total amount of constraint in the dyad, the symmetry of the dyadic constraint, and the interaction between the two.

Because the input-output tables on business transactions are available only at the industry level, studies in which interdependence is a key variable have been forced to operate at the industry level of analysis (Pfeffer and Salancik 1978; Burt 1983; Mizruchi and Koenig 1986). Although the industry level of analysis has proved adequate for the studies that have employed it, I believe that the appropriate unit of analysis for corporate political behavior is the firm. Therefore, in the present study, the measure of constraint will be adapted to the firm level, following studies by Galaskiewicz et al. (1985), Palmer, Friedland, and Singh (1986), and Burt (1987a). See Mizruchi (1989a) for an extensive discussion of this issue.

The Burt measure was computed by summing, for each dyad, the amount of constraint exerted by firm j 's primary industry on firm i 's primary industry, and vice versa. The Galaskiewicz et al. measure was computed by listing all constraint relations between industries in which the two firms are involved and taking the median constraint score. And

⁹ This measure is equivalent to a Pearson product-moment correlation before subtracting the means in computing the sums of squares and cross products. When unit means are small, it is virtually identical to a Pearson correlation. In the present context, unit means are indeterminate because there is no limit on the number of candidates to whom PACs may contribute. Thus, for practical purposes, the means are approximately zero. Substitution of a Pearson correlation (with means based on the total number of candidates registered with the Federal Election Commission) for S_{ij} yielded results virtually identical to those reported below. The two measures are almost perfectly correlated (.995).

Corporations

the Palmer et al. measure was computed by the formula $C_{ij} = (n_{ij} + n_{ji}) / (n_i * n_j)^{1/2}$, where C_{ij} equals the constraint between i and j , n_{ij} and n_{ji} equal the number of significant constraint relations between i and j , and n_i and n_j equal the number of industries in which firms i and j operate.

Because there is little substantive basis on which to decide among these three measures, I constructed a latent variable based on extraction of the first principal component of the correlation matrix among the three measures. The loadings on the first principal component were .760, .502, and .839, respectively, with an eigenvalue of 1.533. Factor scores were employed as the measure of interfirm market constraint.

The degree of asymmetry in the constraint relation was computed by taking, for each firm in the dyad, the ratio of the larger constraint score to the smaller one. Because zero was a possible score, a value of .001 was added to each score before computation. Although adding a constant to a ratio-level variable removes the ratio character of the measure, experimentation with several constants revealed that the value of the constant had little effect on the findings presented below.

Because the three measures of asymmetry were subject to the same conceptual issues as the three measures of constraint, a latent variable based on a linear combination of these three variables was also created. Loadings on the first principal component were .651, .684, and .738, respectively, with an eigenvalue of 1.438. Factor scores were employed as the measure of asymmetry of market constraint. Finally, a multiplicative interaction term for constraint and asymmetry was also created.

Direct and indirect interlocks were calculated by comparing lists of directors between the two firms and between the two firms and each of the 50 largest commercial banks and 20 largest life insurance companies in the United States in 1980. Stockholding relations were determined through comparisons of lists derived from the *CDE Stock Ownership Directory* (Corporate Data Exchange 1981). Geographical proximity was examined for both corporate headquarters and plant locations. For headquarters, I coded a dummy variable for location in the same state. For plants, I coded the number of states that included plants of both firms in the dyad.

Data on campaign contributions were derived from the Federal Election Commission's 1980 tape, which includes all PAC contributions in the 1979-80 election cycle filed with the commission. Data on directors, place of headquarters, plant locations, and the industries in which the firms were involved were derived from *Moody's Industrial Manual* (1981) and *Standard and Poor's Register* (1981). *Fortune* (1981) and *Standard and Poor's* were used to identify firms' primary industries. In a few cases, the *Standard and Poor's* and *Fortune* primary industry classifications differed. The results reported below were based on the *Fortune*

classifications. Substitution of the *Standard and Poor's* classification had no effect on the findings. Interindustry input-output data and constraint scores from 1977 (the closest year to 1980 for which such data are available) were generously provided by Ronald Burt. Burt (1988) has shown that relations between industries in the input-output table are highly stable over time.

RESULTS

Means, standard deviations, and correlations among the variables are presented in table 1. Results of three multiple-regression equations are presented in table 2. Because the units of analysis in this study are the 1,596 dyads created by the relations among 57 firms, the observations are not entirely independent. To deal with this issue, I employed least-squares-with-dummy-variables (LSDV) regression models. This involved the creation of 56 dummy variables, one for each of $N-1$ firms. The correlations in table 1 are partial correlations, with the effects of the firm dummy variables controlled. A brief discussion of the rationale behind the use of this approach is presented in the Appendix. Since the data presented in this analysis were not based on a random sample, the use of statistical significance tests is unnecessary. I have included significance tests for two reasons: first, as a heuristic device for interpreting the strength of relations among variables; second, because my aim is to make generalizations about the causal processes that generated the data (Blalock 1979, p. 242). Whether or not the tests are employed has no effect on the article's conclusions.

Equation (1) of table 2 reveals support for most of the hypotheses presented above. The two strongest predictors of similarity of political behavior were whether the firms were members of the same primary industry and whether their headquarters were located in the same state. The strong effect of primary industry confirms the hypothesis that firms in an industry are likely to exhibit shared political behavior. Surprisingly, the number of common states in which the firms had plants had virtually no effect on the similarity of their contribution patterns (the variable was deleted from the model after it became clear that it had no effect on either the dependent variable or the coefficients of the other predictors). There are at least two possible interpretations of this finding. One possibility is that, because top corporate officials generally live in or near the corporate headquarters city, the proximity coefficient is a reflection of the social interaction among elites in similar locations. This is based on the assumption that social interaction is a source of similar political behavior (see Palmer et al. 1986, p. 786, for a similar argument). It may also reflect a greater concern among corporate officials with the fate of their headquar-

TABLE 1
MEANS, STANDARD DEVIATIONS, AND CORRELATIONS AMONG VARIABLES ($n = 1,596$)^{*}

	Mean	SD	2	3	4	5	6	7	8	9
1. Similarity	.233	.107	.186	.199	.201	.104	.085	.051	.093	-.031
2. Proximity	.081	.274		.094	.028	.070	.086	.071	.093	-.019
3. Same primary industry	.050	.218			.379	.111	.092	-.046	.017	-.108
4. Common industries	.174	.222				.303	.083	-.054	.034	-.088
5. Market constraint†	.000	1.000					.083	-.017	.021	.284
6. Common stockholders‡	1.160	.665						.004	.024	.044
7. Direct interlocks	.048	.241						.181	.003	
8. Indirect interlocks	.403	.719							.023	
9. Asymmetry of constraint†	.000	1.000								

* Partial correlations with firm-specific effects controlled.

† Factor scores.

‡ In logarithms (base e).

TABLE 2
LSDV ESTIMATES OF EFFECTS ON SIMILARITY OF POLITICAL BEHAVIOR

INDEPENDENT VARIABLES	DEPENDENT VARIABLES		
	Equation (1): Similarity	Equation (2): Similarity	Equation (3): Common Industries
Proximity	34.998*** (6.151)	36.248*** (6.425)	.321*** (15.639)
Same primary industry	46.286*** (6.752)	34.906*** (4.844)	
Common industries		41.061*** (5.032)	
Market constraint	5.518** (2.992)	2.417 (1.346)	.062*** (11.784)
Common stockholders	8.196* (2.021)	7.310* (1.816)	
Direct interlocks	9.631 (1.517)	10.919* (1.731)	
Indirect interlocks	7.157** (2.764)	6.611** (2.572)	
Asymmetry	-2.240 (-1.127)		
Constraint* asymmetry	-.959 (-.691)		
R ² (total)711	.715	.373
R ² (residual)084	.096	.212

Note.—Dependent variables: equations (1) and (2), similarity of political behavior $\times 10^3$; equation (3), standardized number of common industries, $N = 1,596$ in all equations. Unstandardized coefficients are reported, with t-statistics in parentheses. Coefficients of firm dummy variables are omitted to conserve space. Total R^2 includes effects of the 56 firm dummy variables. Residual R^2 contains effects based on omission of firm-specific effects from all variables.

*P < .05 (one-tailed test).

**P < .01 (one-tailed test).

***P < .001 (one-tailed test).

Corporations

ters location than with their plant locations (Logan and Molotch 1987; Useem 1984, p. 120).

The hypotheses on the effects of constraint were also confirmed. The market constraint factor was a significant positive predictor of similarity, but the degree of asymmetry in the dyadic constraint relation had no effect. In addition, the level of asymmetry had no effect on the strength of the constraint coefficient (the interaction effect was zero). These findings suggest that the potential for economic coercion is associated with the similarity of political behavior, but the key factor is the existence of constraint in the dyad rather than the relative balance of constraint between the two firms.

Common stock ownership by financial institutions was also positively associated with similarity of political behavior. Although, in the absence of more detailed data on PAC decision-making processes, it is impossible to know whether this represents an exercise of financial influence over the decisions of nonfinancial corporations, it is consistent with the argument of Mintz and Schwartz (1985) that financial institutions play a role in enforcing similar political behavior in the business community.

The findings on interlocking are especially noteworthy. Although direct interlocks among manufacturing firms is positively associated with similarity of political behavior, the effect is not statistically significant. This finding is consistent with those of Mizruchi and Koenig (1986) and Burris (1987; but see Clawson and Neustadt 1989). However, the effect of indirect interlocking through banks and insurance companies is strongly positive. This is consistent with the finding by Mizruchi and Koenig (1988) that the level of indirect interlocking among the largest firms in 25 industries was positively associated with the similarity of their contribution patterns.

Why would firms that were tied indirectly through financial institutions be more likely to behave similarly than firms that were tied directly to one another? One possibility, consistent with the Mintz and Schwartz (1985) model, is that financial institutions are able to use their economic power to achieve political goals, including the ability to dictate, overtly or covertly, the contribution strategies of certain manufacturing firms. In this view, firms that are heavily dependent on financial institutions behave in ways that they believe will win favor with (or avoid the wrath of) officials of these institutions, even in the absence of direct coercion (Whitt 1982, p. 116). Another possible explanation is based on the distinction between cohesion and structural equivalence (White, Boorman, and Breiger 1976). In a series of books and articles, Burt (e.g., 1987b) has found that structural equivalence (the extent to which pairs of units in a social structure share similar relations to other units) is a stronger predictor of the adoption of innovations than is cohesion (which Burt has defined as

"intense, mutual relations"). That is, two individuals who share equivalent positions in a social structure are more likely to exhibit the same behavior than are two individuals who share close relations with each other.

Burt's argument has clear relevance to the findings reported here. Firms that share a direct interlock tie have a slight tendency to contribute to the same candidates, but the effect does not differ significantly from zero. On the other hand, firms that are tied to the same third parties are significantly more likely to contribute to the same candidates. Burt argues that structurally equivalent actors are likely to have competitive relations with one another, for example, when they use "one another to evaluate their relative adequacy" (1987b, p. 1291). According to Burt, "The more similar ego's and alter's relations with other persons are—that is, the more that alter could substitute for ego in ego's role relations . . . —the more likely it is that ego will quickly adopt any innovation perceived to make alter more attractive as the object or source of relations." This point is also applicable to corporate behavior. In a reanalysis of data from an earlier study of corporate philanthropy (Galaskiewicz 1985), Galaskiewicz and Burt (1987) found that structurally equivalent (as opposed to cohesive) corporate officials had similar evaluations of nonprofit organizations. Both Galaskiewicz (1985) and Useem (1984) found that corporate officers were highly sensitive to the ways in which their firms' gifts were perceived by other members of the corporate community. Whitt's (1982) findings suggest that corporate leaders may be equally sensitive to the ways in which their political contributions are perceived. Useem (private communication) has noted that CEOs appear to be concerned that others important to them view them as having "done the right thing" with regard to political contributions. The findings reported here suggest that structurally equivalent firms may adapt their behavior to the perceived preferences of the same set of alters.¹⁰

To what extent is the effect of indirect ties on common contribution patterns contingent on the presence of a direct interlock tie? To examine this, I inserted a multiplicative interaction term into equation (1) of table 2 (the results are not shown here but are available on request from the author). The effect of this variable was virtually zero, while the other coefficients and *t*-statistics remained virtually the same. This indicates that the positive effect of indirect interlocks on similarity of political behavior is unaffected by whether or not a direct tie exists.

¹⁰ Handler and Mulkern (1982, p. 61) report that "pressures felt by CEOs to emulate the example of peers" is a major motive influencing a firm's decision to form a PAC. Note also the similarity between this discussion and the processes described by DiMaggio and Powell (1983).

Corporations

The results presented in equation (1) of table 2 did not include the variable for the standardized total number of common industries between two firms. Equation (2) contains the variables from equation (1) (minus asymmetry and the constraint-asymmetry interaction term) plus the number of common industries. The insertion of this variable has three main consequences. First, the variable is itself a strong predictor of similarity of contribution patterns. Its coefficient exceeds its standard error by a factor of more than five. Second, as one would expect, its inclusion lowers the effect of being in the same primary industry (the latter's β declines from .167 to .126), although same primary industry remains one of the strongest predictors of similarity. Third, insertion of number of common industries washes out the significant effect of market constraint. Its standardized coefficient declines from .077 to .034, and it is no longer statistically significant, although its *t*-statistic remains greater than one. I believe, consistent with the literature on corporate diversification (Chandler 1962; Williamson 1975; Burt 1983), that the number of common industries interprets the effect of market constraint on the similarity of political behavior because firms are likely to acquire firms or subsidiaries in industries that exert constraint over their primary industry. Equation (3) of table 2 shows that primary industry and interfirm market constraint are strong predictors of the standardized number of common industries. In fact, the indirect effect of constraint on similarity through common industries has a larger, although still small, effect than the direct effect of constraint on similarity.

Because interlocks and stock ownership have been shown to be partly determined by resource dependencies (Allen 1974; Pfeffer and Salancik 1978; Pennings 1980; Burt 1983; Mizruchi and Stearns 1988), I constructed a path model with common stock ownership and interlocks as intervening variables (I have presented a detailed description of these results [Mizruchi 1989a], which is available on request). Following Mizruchi and Koenig (1986), I found that market constraint had no effect on either direct or indirect interlocking, although it did have an effect on common stock ownership. The null finding on the effect of market constraint is inconsistent with Burt's (1983) results at the industry level. Burt found that industries in which constraint relations exist are more likely to have interlock ties. Attempts by Galaskiewicz et al. (1985) and Palmer et al. (1986) to replicate this finding at the firm level were unsuccessful, although Burt's (1987a) attempt was successful. In order to see whether effects could be observed for the individual rather than the composite measures of constraint, I estimated regression equations for direct and indirect interlocks using each of the three operationalizations discussed earlier (the Galaskiewicz et al., Palmer et al., and Burt measures). The effect of constraint was zero in all six equations. I also computed these six

American Journal of Sociology

equations using OLS regression rather than LSDV. I failed to find a significant effect on direct interlocking for any of the three measures of constraint. I did, however, find that the constraint between the firm's primary industries (the Burt interfirm measure) was a significant positive predictor of indirect interlocks. Since firms in the same primary industry have a positive probability of being indirectly interlocked, this finding could be viewed as consistent with Burt's.

The main conclusion to be drawn from the models of stockholding and interlocking is that although two of the three variables are significant predictors of similarity of political behavior, none appears to interpret the effects of market constraint on similarity.

CONCLUSION

The aim in this paper has been to examine the conditions under which similar political behavior occurs between major corporations. A model was presented, based on elements of the resource-dependence and social class models of intercorporate relations, to examine the effects of various organizational and social network variables on behavioral similarity. It was argued that when a market relation between two firms is characterized by the existence of market constraint, the firm or firms subject to this constraint are under pressure, either overtly or covertly, to coalesce over political issues. The model was tested through an examination of the determinants of common congressional campaign contributions among the 1,596 dyads created by 57 large manufacturing firms.

The results provide support for the main tenets of the model. Both organizational and social network factors played independent roles in predicting similarity of political behavior. Chief among these were the effects of market constraint and common industries between pairs of firms and the effects of common stockholding by financial institutions and indirect interlocks through these institutions. Moreover, the results suggest that both the organizational and social network effects appear to be primarily direct rather than indirect. The effect of common stock ownership, although partly determined by market constraint, is primarily an independent one. The indirect interlock effect is almost entirely independent. This strongly suggests that, to the extent that director interlocks affect common political behavior, it is primarily as a social network phenomenon rather than as a consequence of market constraint. The effect of market constraint on similarity of political behavior is interpreted not by its effect on director interlocks but by its effect on participation in common industries.

In an earlier study based on a small sample of industry-level dyads (Mizruchi and Koenig 1986), market constraint was positively associated

Corporations

with similar political behavior, but interlocking had no effect. The findings presented here, based on a large sample of firm-level dyads and a more fully specified model, both extend and modify the conclusions reached in the earlier study. Consistent with the previous results, market constraint was positively associated with similar political behavior. Direct interlocking was not strongly associated with similar behavior, but the effect in equation (2) of table 2 was marginally significant, and, unlike in the earlier study, the coefficient was positive. In addition, I found, consistent with more recent results (Mizruchi and Koenig 1988), that indirect interlocking through financial institutions was significantly associated with similar political behavior. This finding suggests, in accordance with much recent network literature, that actors with relations to the same third parties may be more likely to behave similarly than those with direct relations to one another.

Most of the debate about the exercise of power in industrialized Western societies has taken place at a high level of abstraction. The preoccupation with whether business is capable of resolving its disputes has stimulated a great deal of useful research, but researchers in political sociology have been unable to resolve the controversy definitively. As with other important debates that have occurred on a relatively abstract level, such as those between functionalists and conflict theorists two decades ago, it is unclear whether a clear solution is possible. Rather than continue to debate whether elites are unified or divided, it may be more useful to try to understand the conditions under which unity and conflict occur. This study has taken one step in that direction, but considerable work remains to be done.

First, several aspects of corporate political action need to be examined. To what extent do the factors that predict contributions to the same candidates also predict contributions to candidates of the same party or similar ideological positions? Do firms in similar positions or with similar interests focus on the same types of candidates but in different races? Second, although Clawson et al. (1986) found that head-to-head opposition between firms in particular elections is relatively rare, to what extent can the political opposition that does exist be explained by the absence of the variables examined in the present study? Are market constraint and indirect interlocks, for example, associated with low levels of opposition? How frequently do members of the same industry oppose one another? Third, additional areas of corporate political behavior warrant examination. A key one, which encompasses a wide range of corporate political activities, is business lobbying of government officials. Such an examination would widen the scope of the study and would provide a partial validity check on the use of common contribution patterns as an indicator of similar political interests. Finally, an examination of business attitudes

American Journal of Sociology

as well as behavior would prove useful, especially on the relation between common attitudes and common behavior (see Barton 1985; Ornstein 1985; Whitt and Mizruchi 1986, for studies of elite attitudes). In fact, a series of in-depth interviews with CEOs and PAC officials might help researchers to shed light on the processes by which variables such as market constraint and director interlocks influence similar political behavior. Regardless of the indicators that are employed, it is worth restating that the model presented here was designed to apply to the corporate community as a whole. The dyadic approach was a useful mechanism for ensuring sufficient degrees of freedom. But the analysis must move to the triadic, cluster, and system-wide levels (see Neustadt and Clawson 1988, for an example).

This article has demonstrated the organizational and social network bases of one aspect of corporate political behavior. Ultimately, however, the value of this work will be determined by its ability to help us understand the workings of power in advanced capitalist societies. Jacobs (1988) has examined the hypothesis that the level of economic resources in the business community is associated with corporate political power. He found a negative relation between the resources of large corporations (measured by economic concentration) and corporate tax rates over time, a result that he viewed as consistent with his hypothesis. Several theorists have noted, however, that there is no necessary connection between high levels of resources and political power. As Dahl (1961) has pointed out, the degree of organization is a crucial intervening variable. The model presented here may help specify the processes by which corporate resources are translated into effective political power. The extent to which it proves useful in doing so awaits further study.

APPENDIX

Statistical Model

The dependent variable in this study is normally distributed. Therefore, ordinary least-squares (OLS) regression analysis would appear to be an appropriate technique. Because my units of analysis are the 1,596 dyads created by relations among 57 firms, however, the observations are not entirely independent. Each firm will appear in 56 separate observations. This raises the possibility of autocorrelation in the regression results. There is no agreed-upon approach to handling network autocorrelation.

One possibility is a spatial autocorrelation model that has been developed for use with cross-sectional data (see Cliff and Ord 1981; Doreian 1981). Dow et al. (1984) discuss three approaches to calculating regression equations with this model. I have employed one of these approaches in

Corporations

another study (Mizruchi and Koenig 1988), and it appears to be well suited to the present study. The main problem is computational. Dow et al.'s illustrations, as well as the Mizruchi and Koenig study, had sample sizes of under 100. Since spatial autocorrelation models involve the use of a square matrix of unit-by-unit overlaps, we would be faced with the inversion of a $1,596 \times 1,596$ matrix. Because of the enormous amount of storage capacity that would be necessary to handle a matrix of this size, this technique is not feasible for the present analysis.

An alternative is to employ least squares with dummy variables (see Hannan and Young [1977] for a discussion applied to pooled cross-sectional time-series models). In LSDV, dummy variables are created for 56 of the 57 firms. For each dyad, the dummies for the two firms are coded "1," and all other dummies are coded "0." The standard matrix equation for OLS regression is $\mathbf{Y} = \mathbf{XB} + \mathbf{e}$, where \mathbf{e} is an $N \times 1$ vector of residuals. With the creation of the dummy variables, the potentially autocorrelated disturbances are shifted out of \mathbf{e} and into \mathbf{X} (Hannan and Young 1977, pp. 64–65). The remaining terms in \mathbf{e} now satisfy the criteria for OLS regression. Hannan and Young's simulation results suggest that LSDV yields consistent and efficient estimates.

The "true" number of degrees of freedom in the present analysis is somewhere between the number of nodes minus one (56) and the number of dyads minus one (1,595). The extent to which the degrees of freedom approach the number of dyads depends on the level of autocorrelation. If the autocorrelation is removed, the observations can be treated as if they were independent (Hannan and Young 1977, p. 59; Lincoln 1984, p. 57).

REFERENCES

Alford, Robert R., and Roger Friedland. 1985. *Powers of Theory: Capitalism, the State, and Democracy*. New York: Cambridge University Press.

Allen, Michael P. 1974. "The Structure of Interorganizational Elite Cooptation: Interlocking Corporate Directorates." *American Sociological Review* 39:393–406.

Baran, Paul A., and Paul M. Sweezy. 1966. *Monopoly Capital*. New York: Monthly Review.

Barton, Allen H. 1985. "Determinants of Economic Attitudes in the American Business Elite." *American Journal of Sociology* 91:54–87.

Bauer, Raymond A., Ithiel de Sola Pool, and Lewis Anthony Dexter. 1972. *American Business and Public Policy: The Politics of Foreign Trade*. New York: Atherton.

Blalock, Hubert M., Jr. 1979. *Social Statistics*, 2d ed. New York: McGraw-Hill.

Blau, Peter M. 1964. *Exchange and Power in Social Life*. New York: Wiley.

Burris, Val. 1987. "The Political Partisanship of American Business: A Study of Corporate Political Action Committees." *American Sociological Review* 52:732–44.

Burt, Ronald S. 1983. *Corporate Profits and Cooptation: Networks of Market Constraints and Directorate Ties in the American Economy*. New York: Academic.

_____. 1987a. "Broken Ties and Corporate Markets." Unpublished manuscript. Columbia University, Department of Sociology.

American Journal of Sociology

_____. 1987b. "Social Contagion and Innovation: Cohesion versus Structural Equivalence." *American Journal of Sociology* 92:1287–1335.

_____. 1988. "The Stability of American Markets." *American Journal of Sociology* 94:356–95.

Carnoy, Martin. 1984. *The State and Political Theory*. Princeton: Princeton University Press.

Chandler, Alfred D., Jr. 1962. *Strategy and Structure*. Cambridge: MIT Press.

Clawson, Dan, and Alan Neustadt. 1989. "Interlocks, PACs, and Corporate Conservatism." *American Journal of Sociology* 94:749–73.

Clawson, Dan, Alan Neustadt, and James Bearden. 1986. "The Logic of Business Unity: Corporate Contributions in the 1980 Election." *American Sociological Review* 51:797–811.

Cliff, Andrew, and Keith Ord. 1981. *Spatial Processes: Models and Applications*. London: Pion.

Cook, Karen S., and Richard M. Emerson. 1978. "Power, Equity, and Commitment in Exchange Networks." *American Sociological Review* 43:721–39.

Corporate Data Exchange. 1981. *CDE Stock Ownership Directory: Fortune 500*. New York: Corporate Data Exchange.

Coser, Lewis A. 1956. *The Functions of Social Conflict*. New York: Free Press.

Dahl, Robert A. 1961. *Who Governs?* New Haven: Yale University Press.

Dahrendorf, Ralf. 1959. *Class and Class Conflict in Industrial Society*. Stanford: Stanford University Press.

DiMaggio, Paul J., and Walter W. Powell. 1983. "The Iron Cage Revisited: Institutional Isomorphism and Collective Rationality in Organizational Fields." *American Sociological Review* 48:147–60.

Doreian, Patrick. 1981. "Estimating Linear Models with Spatially Distributed Data." Pp. 359–88 in *Sociological Methodology, 1981*, edited by Samuel Leinhardt. San Francisco: Jossey-Bass.

Dow, Malcolm M., Michael L. Burton, Douglas R. White, and Karl P. Reitz. 1984. "Galton's Problem as Network Autocorrelation." *American Ethnologist* 11:754–70.

Emerson, Richard M. 1962. "Power-Dependence Relations." *American Sociological Review* 27:32–41.

Etzioni, Amitai. 1984. *Capital Corruption: The New Attack on American Democracy*. New York: Harcourt Brace Jovanovich.

Fortune. 1981. "The 500 Largest Industrial Corporations. *Fortune* 105, no. 7 (May 4): 324–43.

Galaskiewicz, Joseph. 1985. *Social Organization of an Urban Grants Economy*. Orlando, Fla.: Academic.

Galaskiewicz, Joseph, and Ronald S. Burt. 1987. "Interorganization Contagion and Corporate Philanthropy." Unpublished manuscript. University of Minnesota, Department of Sociology.

Galaskiewicz, Joseph, Stanley Wasserman, Barbara Rauschenbach, Wolfgang Bielefeld, and Patti Mullaney. 1985. "The Impact of Corporate Power, Social Status, and Market Position on Corporate Interlocks in a Regional Network." *Social Forces* 64:403–31.

Handler, Edward, and John Mulkern. 1982. *Business in Politics*. Lexington, Mass.: Lexington.

Hannan, Michael T., and Alice A. Young. 1977. "Estimation in Panel Models: Results on Pooling Cross-Sections and Time Series." Pp. 52–83 in *Sociological Methodology, 1977*, edited by David R. Heise. San Francisco: Jossey-Bass.

Herman, Edward S. 1981. *Corporate Control, Corporate Power*. New York: Cambridge University Press.

Corporations

Jacobs, David. 1988. "Corporate Economic Power and the State: A Longitudinal Assessment of Two Explanations." *American Journal of Sociology* 93:852-81.

Koenig, Thomas, and Robert Gogel. 1981. "Interlocking Corporate Directorates as a Social Network." *American Journal of Economics and Sociology* 40:37-50.

Kouzi, Anthony, and Richard Ratcliff. 1985. "Political Contributions and Corporate Influence." Paper presented at the annual meeting of the Eastern Sociological Society, Philadelphia.

Laumann, Edward O., and David Knoke. 1987. *The Organizational State: Social Choice in National Policy Domains*. Madison: University of Wisconsin Press.

Laumann, Edward O., and Peter V. Marsden. 1982. "Microstructural Analysis in Interorganizational Systems." *Social Networks* 4:329-48.

Laumann, Edward O., Peter V. Marsden, and Joseph Galaskiewicz. 1977. "Community-Elite Influence Structures: Extension of a Network Approach." *American Journal of Sociology* 83:594-631.

Lincoln, James R. 1984. "Analyzing Relations in Dyads." *Sociological Methods and Research* 13:45-76.

Logan, John R., and Harvey L. Molotch. 1987. *Urban Fortunes: The Political Economy of Place*. Berkeley and Los Angeles: University of California Press.

Mariolis, Peter. 1975. "Interlocking Directorates and Control of Corporations." *Social Science Quarterly* 56:425-39.

Merton, Robert K. 1936. "The Unanticipated Consequences of Purposive Social Action." *American Sociological Review* 1:894-904.

Mills, C. Wright. 1956. *The Power Elite*. New York: Oxford University Press.

Mintz, Beth, and Michael Schwartz. 1985. *The Power Structure of American Business*. Chicago: University of Chicago Press.

Mizruchi, Mark S. 1982. *The American Corporate Network, 1904-1974*. Beverly Hills, Calif.: Sage.

_____. 1987. "Why Does Business Stick Together? An Interorganizational Theory of Class Cohesion." Pp. 204-18 in *Power Elites and Organizations*, edited by G. William Domhoff and Thomas R. Dye. Beverly Hills, Calif.: Sage.

_____. 1989a. "Market Relations, Interlocks, and Corporate Political Behavior." Preprint 118. Columbia University, Center for the Social Sciences.

_____. 1989b. "Cohesion, Structural Equivalence, and Similarity of Behavior: An Approach to the Study of Corporate Political Power." Preprint 119. Columbia University, Center for the Social Sciences.

Mizruchi, Mark S., and Thomas Koenig. 1986. "Economic Sources of Corporate Political Consensus: An Examination of Interindustry Relations." *American Sociological Review* 51:482-91.

_____. 1988. "Economic Concentration and Corporate Political Behavior: A Cross-Industry Comparison." *Social Science Research* 17:287-305.

Mizruchi, Mark S., and Linda Brewster Stearns. 1988. "A Longitudinal Study of the Formation of Interlocking Directorates." *Administrative Science Quarterly* 33:194-210.

Moody's Industrial Manual. 1981. New York: Moody's Investors Service.

Neustadt, Alan, and Dan Clawson. 1988. "Corporate Political Groupings: PAC Contributions to the 1980 Congressional Elections." *American Sociological Review* 53:172-90.

Ornstein, Michael. 1985. "The Political Ideology of the 'Inner Group' of Canadian Capital." *Journal of Political and Military Sociology* 13:219-37.

Palmer, Donald, Roger Friedland, and Jitendra V. Singh. 1986. "The Ties That Bind: Organizational and Class Bases of Stability in a Corporate Interlock Network." *American Sociological Review* 51:781-96.

Pennings, Johannes M. 1980. *Interlocking Directorates*. San Francisco: Jossey-Bass.

American Journal of Sociology

Pfeffer, Jeffrey. 1987. "A Resource Dependence Perspective on Intercorporate Relations." Pp. 25–55 in *Intercorporate Relations: The Structural Analysis of Business*, edited by Mark S. Mizruchi and Michael Schwartz. New York: Cambridge University Press.

Pfeffer, Jeffrey, and Gerald R. Salancik. 1978. *The External Control of Organizations: A Resource Dependence Perspective*. New York: Harper & Row.

Rothenberg, Stuart, and Richard R. Roldan. 1983. *Business PACs and Ideology*. Washington, D.C.: Free Congress Research and Education Foundation.

Sabato, Larry J. 1984. *PAC Power: Inside the World of Political Action Committees*. New York: Norton.

Schumpeter, Joseph. 1942. *Capitalism, Socialism, and Democracy*. New York: Harper.

Standard and Poor's Register of Corporations, Directors, and Executives. 1981. New York: Standard & Poor's.

Useem, Michael. 1984. *The Inner Circle*. New York: Oxford University Press.

White, Harrison C., Scott A. Boorman, and Ronald L. Breiger, 1976. "Social Structure from Multiple Networks. I. Blockmodels of Roles and Positions." *American Journal of Sociology* 81:730–80.

Whitt, J. Allen. 1982. *Urban Elites and Mass Transportation*. Princeton: Princeton University Press.

Whitt, J. Allen, and Mark S. Mizruchi. 1986. "The Local Inner Circle." *Journal of Political and Military Sociology* 14:115–25.

Williams, Robin M., Jr. 1970. *American Society: A Sociological Interpretation*, 3d ed. New York: Knopf.

Williamson, Oliver E. 1975. *Markets and Hierarchies: Analysis and Antitrust Implications*. New York: Free Press.

Zald, Mayer N. 1970. "Political Economy: A Framework for Comparative Analysis." Pp. 221–61 in *Power in Organizations*, edited by Mayer N. Zald. Nashville: Vanderbilt University Press.

Commentary and Debate

To conserve space for the publication of original contributions to scholarship, the comments in this section must be limited to brief critiques. They are expected to address specific errors or flaws in articles and reviews published in the *AJS*. Comments on articles are not to exceed 1,500 words, those on reviews 750 words. Longer or less narrowly focused critiques should be submitted as articles. Authors of articles and reviews are invited to reply to comments, keeping their replies to the length of the specific comment. The *AJS* does not publish commenters' rebuttals to authors' replies. We reserve the right to reject inappropriate or excessively minor comments.

SETTING THE RECORD STRAIGHT ON ORGANIZATIONAL ECOLOGY: REBUTTAL TO YOUNG¹

In its July 1988 issue (94:1–24), *AJS* published a commentary on some of our studies: "Is Population Ecology a Useful Paradigm for the Study of Organizations?" We are grateful to *AJS* for focusing attention on our work.² Unfortunately, the commentary draws a caricature of our contribution to organizational ecology and confuses numerous issues. Limitations of space preclude correcting all the misunderstandings and misattributions here. However, we do wish to correct the misleading view that the commentary is a systematic and comprehensive evaluation of the population ecology of organizations, as its title, introduction, and conclusion suggest.

Setting the record straight requires that we address two broad issues. Before a theoretical strategy can be evaluated, attention must be paid to

¹ Preparation of this essay was supported by National Science Foundation grants SES-8811489 and SES-8809006. We are grateful to numerous colleagues for offering helpful suggestions. A portion of this essay is adapted from the preface of *Organizational Ecology* (Hannan and Freeman 1989).

² We regret that the author refused our request for a copy of the manuscript and that *AJS* also declined to let us see the manuscript before publication. We were thus not permitted to reply in the same issue. [It is *AJS* policy not to release the manuscript of an article before publication without the author's permission.—ED.]

Permission to reprint a comment printed in this section may be obtained only from the author.

American Journal of Sociology

its goals and scope. Yet the critique made no effort to address the motivations and goals of our approach. Indeed, it takes as its premise the incorrect view that we have attempted to reduce organizational processes to biological ones, that we seek a mapping between the organizational and biotic worlds.

A responsible assessment of the “usefulness” of a program of theory and research also requires consideration of the full breadth of the research it produces. But, in trying to make the best of a weak case that our work is narrow and has had little effect, the commentary ignores our recent empirical work and that of many other scholars who have refined and extended our approach.³ In response, we discuss five lines of research that have clarified and advanced our understanding of the ecology of organizations. But first we turn to the goals of the approach.

Motivations

The ecology of organizations is an approach to the macrosociology of organizations that builds on general ecological and evolutionary models of change. The goal of this perspective is to understand the forces that shape organizational structures over long time spans. Our interest in developing an ecology of organizations began in the mid-1970s at a time when the sociology of organizations was shifting toward an emphasis on the effects of environments on the structure and functioning of organizations. Most theorists and researchers in this period treated organizations as rational, flexible, and speedy adapters to changing environmental circumstances and explained variability in structures in terms of adaptations to environmental constraint by individual organizations or prescient managers.

We did not find this approach to be useful in explaining the structures we observed. The organizations we observed did not seem to have unitary, stable preference structures and simple mechanisms for implementing them in the face of changing conditions. In our experience, subunits and coalitions commonly engaged in political contests for control over decisions and resources. Such political processes, along with various kinds of sunk costs and legitimacy constraints, caused these organizations to be anything but flexible and quick in collective response to changing opportunities and constraints in the environment. Put simply, we thought it was a mistake to build models of organizational change that rely on anthropomorphic images of organizations or on heroic images of man-

³ The critique treats unfairly other scholars working in this tradition. Even supposing that a serious flaw had been identified in our work, it does not follow that research by other scholars who use a similar perspective must also be flawed.

Commentary and Debate

agers. Hence, we have attempted to build a perspective that treats organizations as complex systems with strong limitations on flexibility and speed of response ("structural inertia").

Following the proclivity to think anthropomorphically about organizations, the literature in this period also used what we have called a focal organization perspective. That is, theorists and researchers sought to describe and explain the action of single organizations facing specified environmental problems. But this perspective too seemed problematic. The success or failure of any tactic or strategy for dealing with an environmental problem presumably depends on the tactics and strategies adopted by the other similar (and dissimilar) organizations in the system. Therefore, we thought it would be productive to shift the level of analysis to the population level. This means considering actions of all organizations of a type within a bounded system.

We also concluded that the organizational theory of the 1970s was leading the field to an isolated position within sociology. Both the tendency to reason anthropomorphically about organizations and the focal organization perspective greatly limited the value of organizational theory for explaining large-scale social change. Indeed a major thrust of our work has been to build bridges with other branches of macrosociological theory and research. For example, we have developed implications of organizational ecology for processes of large-scale social change, social movements, and social inequality (Hannan and Freeman 1986; Hannan 1986a, 1988a, 1988d).

In preparing to develop a different kind of theory of organizational change, we considered two kinds of analytic structures. The first was the theory of the firm in competitive markets. This collection of theories has the appropriate population focus. Its distinguishing characteristic is an emphasis on the consequences of simultaneous actions of all parties in a market. However, this approach relies on theories of action and of organization that seemed to share many of the defects of contemporary organization theory. Firms were viewed as rational, unitary actors with broad behavioral repertoires. So following the example of formal models of the behavior of firms seemed to deal with only one of the two broad problems that concerned us.

A second analytic structure, ecological models of population and community structure and dynamics, seemed more promising. These theories sought to explain how selection processes shape adaptation at the population level to environmental variations. The individual actors, biotic creatures, are assumed to have relatively fixed repertoires of action. So the motor of change is selection—the excess of births over deaths of actors that possess certain fixed strategies.

We proposed an approach to analyzing organizational change using

models drawn from population ecology (Hannan and Freeman 1977, 1984). In doing so, we called attention to the strength of inertial forces on core features of organizational structure. We did not argue that individual organizations never change, only that previous theory had overemphasized adaptive change as a process generating variability in organizational characteristics of interest. Indeed, we specified inertia in terms of relative rates of change: "To claim that organizational structures are subject to strong inertial forces is not the same as claiming that organizations never change. Rather, it means that organizations respond relatively slowly to the occurrence of threats and opportunities in their environments. Therefore structural inertia must be defined in relative and dynamic terms. It refers to comparisons of the typical rates of change. . . . In particular, structures of organizations have high inertia when the speed of reorganization is much lower than the rate at which environmental conditions change" (Hannan and Freeman 1984, p. 151).

Despite the contrary claims in the commentary, we did not seek to use biological theory to explain organizational change. Nor did we propose to develop metaphors between biotic populations and organizational populations. Rather, we wanted to explain a variety of sociological phenomena that could not be explained satisfactorily with existing social science theories. We relied on models from population biology because these models appeared to clarify the social processes of interest. As we explained initially, in describing our use of a classic model developed by mathematical biologists Alfred Lotka and Vito Volterra: "No known population (of animals, or of organizations) grows in exactly the manner specified by this mathematical model. . . . What the equations do is to model the growth path of populations that exist on finite resources in a closed system. . . . Neither the protozoa nor the bureaucracies behave exactly as the model stipulates. The model is an abstraction that will lead to insight whenever the stated conditions are approximated" (Hannan and Freeman 1977, p. 961).

We have adapted ecological models to sociological uses and have changed them in the process.⁴ It should be clear that we use ecological models as points of departure for modeling and analyzing *social* processes. Consider our approach to studying the effects of intrapopulation competition on rates of founding and mortality in organizational popu-

⁴ Thus, we do not assume that organizational and ecological processes operate according to the same laws. Consider, e.g., the issue of age dependence in mortality rates. The commentary raises as a crucial problem the finding that mortality rates of organizations decline with age, as we discuss below, but mortality rates of most biotic populations rise after some age. Only theorists interested in mechanistic reductions of one science to another would find this difference troubling. Instead, we have developed a *sociological* explanation for the distinctive pattern of age dependence in organizational mortality rates (Hannan and Freeman 1984).

Commentary and Debate

lations. Standard population-biology arguments, as exemplified by the Lotka-Volterra model of competitive interactions, assume that growth in numbers depresses birth rates and raises mortality rates and that these effects are approximately linear. However, we think that these competitive effects are partially offset by an opposing process of institutionalization, which is clearly irrelevant in biotic applications. Our model of competition and legitimization holds that the effect of density on the vital rates of organizational populations is nonmonotonic—not linear (Hannan 1986b; Hannan and Freeman 1988a). Our empirical studies highlight this difference, as we describe below.

Because social systems are complex and because social processes are often highly unstable, the equilibrium assumptions usually employed by biologists and organizational analysts do not seem appropriate to analyzing change in the world of organizations. Our work focuses on dynamics, especially on the local dynamics of processes that shape rates of entry and exit in populations of organizations. In studying dynamics, we consider entire populations of organizations over the full histories of the populations. This strategy of research departs radically from most research on organizations, which tends to focus on the largest and longest-lived organizations and on cross sections or very short time series.

We have departed from a long tradition in organizational analysis separating theory from empirical research. We have developed tight links between theory, models, and empirical research. A key component in this program is methodological. Because we distrust equilibrium assumptions in organizational research, as noted above, we have sought to use models and methods appropriate for analyzing processes of change. In particular, we have relied on the methods of event-history analysis as a framework within which parameters of dynamic models of ecological processes can be estimated.⁵

In moving toward an ecology of organizations, we were influenced by Amos Hawley's (1950, 1968) neoclassical theory of human ecology. This theory, built on the bioecology of the 1930s and 1940s, sought to explain patterns of adaptation of human communities to ecological settings. It relied on a principle of isomorphism: in equilibrium "units subjected to the same environmental conditions, or to environmental conditions as mediated by a given key unit, acquire a similar form of organization"

⁵ The commentary complains that we have not supplied the full details of our methods. This is not so. Tuma, Hannan, and Groeneveld (1979), Tuma and Hannan (1984), and Hannan and Tuma (1985) have presented full details on the approach and on its application to the analytic questions we address, and the empirical papers on organizational ecology provide appropriate citations. Moreover, detailed descriptions of our research designs are available (Hannan 1980, 1988b; Hannan and Freeman 1989, chap. 7).

American Journal of Sociology

(Hawley 1968, p. 334). We thought that the organization theorists of the 1970s were searching for such a principle but lacked the rigorous logic of Hawley's theory. We also thought that Hawley's approach needed to be extended in two ways if it were to apply generally to organization-environment relations. First, isomorphism processes needed to be explored dynamically because organizational populations are rarely observed in equilibrium. Second, the concept of isomorphism had to be generalized to apply to settings in which the environment is spatially and temporally heterogeneous and thus poses problems of inconsistent adaptive demands. A major thrust of our theoretical work is to develop such extensions (see esp. Brittain and Freeman 1980; Freeman and Hannan 1983; Hannan and Freeman 1989).

Our thinking was also influenced strongly by Arthur Stinchcombe's (1965) insightful suggestion that cohorts of organizations are "imprinted" with the social, cultural, and technical features that are common in the environment when the cohort is founded. Imprinted characteristics are highly resistant to change. So the current characteristics of populations of organizations reflect historical conditions at the time of founding rather than recent adaptations. We thought that Stinchcombe was right about the historical facts. And we have suggested that his reasoning was strongly consistent with a population-ecology theory of organizational change. Developing the implications of this argument led us to emphasize rates of founding and rates of mortality in organizational populations and to consider processes over long historical periods.

Our ecological evolutionary approach directs attention primarily to organizational diversity. It seeks to answer the question, Why are there so many (or so few) kinds of organizations? An ecology of organizations seeks to understand how social conditions affect the rates at which new organizations and new organizational forms arise, the rates at which organizations change forms, and the rates at which organizations and forms die out. In addition to focusing on the effects of larger social, economic, and political systems on these rates, an ecology of organizations also emphasizes the interactions that take place within and between organizational populations. An extended treatment of the theoretical approach is now available (Hannan and Freeman 1989).

The commentary makes much of the fact that the boundaries of organizational populations are sometimes fuzzy and seems to suggest that the problem lies with our theory rather than with social reality.⁶ All sociology

⁶ It also gives the misleading impression that issues of taxonomy are simple ones in biotic applications. Our reading of the relevant literature does not support this view. Issues of empirical classification of biotic forms are still hotly debated among biologists. It does not seem to have been the case that taxonomic issues had to be solved before theoretical progress was possible.

Commentary and Debate

has difficulty in specifying boundaries of social systems. For instance, the subtle, shifting nature of the boundaries of social classes, occupations, social movements, interest groups, elites, and communities is well known. More to the point of the commentary, such problems are no more pressing in our work than in other kinds of organizational theory and research. In particular, issues of boundary specification are equally vexing whether the focus is on populations of organizations, organizational "sets," organizational "fields," or "societal sectors," to name some prominent candidates for a unit of analysis in the study of organization-environment relations (see Scott 1987, chap. 6).

We have proposed two approaches to identifying and bounding organizational populations. The first uses information on the goals, strategies, and structures of individual organizations (Hannan and Freeman 1977, 1984). The second uses information on the social processes that create and erode social boundaries (including such processes as technical change, collective action, legitimization, and mobility of persons among organizations) in order to identify bounded populations (Hannan and Freeman 1986). Researchers have used a variety of ways to implement these ideas empirically. We are still learning about the advantages and disadvantages of the alternatives. So, as is the case in virtually all sociology, the question of how to define boundaries around systems is partly open. However, the existence of open questions does not imply logical incoherence.

The critique concludes that little has been accomplished over the past 10 years and that the program of research ought to end. Not surprisingly, we reach a different conclusion. Our strategy has been to attempt to develop theories and methods in parallel so that we could set the stage for a broad effort to evaluate the empirical status of the theoretical program. If our approach to studying organizations was to prove useful, and be used, the theories had to be clear in their empirical implications and the methodology had to be well developed and accessible. The recent surge of population-ecology research using data on diverse kinds of organizational populations testifies that our theoretical approach has empirical import and that problems of definition and conceptualization are not insoluble. We turn next to a brief review of this work.

Research Agendas

The commentary claims that organizational ecologists have not succeeded in studying competition processes and that ecological research on organizations can consider only a narrow range of explanatory variables. These criticisms are far from the mark. Results of five lines of research, which

American Journal of Sociology

have developed along the lines proposed in our early statement of the approach (Hannan and Freeman 1977), speak clearly to these criticisms.⁷

The first line examines competition among interacting populations of organizations using variations on Lotka-Volterra (LV) models. Nielsen and Hannan (1977) and Carroll (1981) analyzed the expansion and contraction of populations of educational organizations. Brittain and Wholey (1988) have analyzed interactions among subpopulations of semiconductor manufacturers. McPherson (1983) has analyzed niche overlap and competition of voluntary associations using the equilibrium implications of the LV model, and McPherson and Smith-Lovin (1988) have replicated this study with data from five nations.

A second thrust considers the ecological implications of aging of organizations. Although there had been speculation that organizations face a "liability of newness" (Stinchcombe 1965), only organizational ecologists addressed this issue using appropriate data and methods. Carroll and Delacroix (1982) found a liability of newness for newspapers in Argentina and Ireland during the 19th and 20th centuries, and Carroll (1983) found a similar pattern in analyzing data on 56 populations as diverse as retail stores, chemical manufacturers, and saloons. Freeman, Carroll, and Hannan (1983) explored whether this apparent effect of aging might be an artifact due to the effect of size on mortality (and an association of size with age). Analysis of populations of newspaper firms, semiconductor manufacturers, and labor unions reveals that adjusting for size does not diminish the effect of aging on mortality. Hannan and Tuma (1985) and Hannan (1988c) show that this conclusion holds even when the effects of unobserved heterogeneity are taken into account, which is important because such heterogeneity alone can explain the observed regularity.

Contrary to the claims of the critique, these studies *do* distinguish multiple causes of mortality when the data permit fine distinctions. So, for example, our studies of the mortality of labor unions have analyzed the processes affecting three "competing risks" of mortality: disbanding, absorption by a dominant union, and merger with one or more unions. We find that these various processes differ—they have different dynamics and are affected by different structural and environmental conditions (Hannan 1988c; Hannan and Freeman 1989).

⁷ The commentary considered only five empirical papers: Freeman and Brittain (1977), Nielsen and Hannan (1977), Hannan and Freeman (1978), Freeman, Carroll, and Hannan (1983), and Freeman and Hannan (1983). Two of these, Freeman and Brittain (1977) and Hannan and Freeman (1978), address only the internal dynamics of organizations, not organizational ecology. Readers might note that many of the papers discussed below had already been published when the critique appeared—Hannan and Freeman (1987) is particularly relevant. Most of the others had been presented at professional meetings and circulated widely in technical report format and were available for consideration.

Commentary and Debate

Third, ecological research considers the effects of environmental turbulence on rates of founding and mortality. Carroll and Delacroix (1982) and Delacroix and Carroll (1983) find that the founding rates and mortality rates of newspapers rise during periods of political turmoil and revolutions. This finding has been replicated in studies of newspaper publishers in San Francisco, 1840–1975 (Carroll and Huo 1986; Carroll 1987), of similar firms in Finland, 1771–1963 (Amurgey, Lehisalo, and Kelly 1988), and also of ethnic newspapers in the United States, 1880–1915 (West and Olzak 1987). There is interesting confluence of organizational ecology and resource-mobilization theory on this point.⁸ Periods of political turmoil entail changes in balances of power among contending groups. Newly contending groups apparently create newspapers in order to clarify and spread their views during such periods. Interestingly, newspapers founded during periods of political turmoil have higher than average rates of mortality.

Other kinds of environmental change examined include technical change in such diverse industries as minicomputers, plate glass, and cement (Anderson 1988), telephone (Barnett 1988), and semiconductor manufacturing (Freeman and Hannan 1987*b*, 1987*c*). Other studies have examined the effects of legal changes in the status of labor unions (Hannan and Freeman 1987, 1988*b*), changes in funding for social services (Singh, House, and Tucker 1986; Singh, Tucker, and House 1986), and changes in government regulation of the telephone industry (Barnett and Carroll 1987). Our studies of the foundings and mortality of labor unions estimated effects of variations in immigration, wages, unemployment rates, GNP, business failure rates, and capital investment. Indeed, most of the analyses described here include the effects of environmental conditions, time trends or period effects, aging, and competitive interactions within and between populations.

Fourth, we have explored why one form of organization has not expanded to take on all or most of the activities of modern societies. We address this question by considering trade-offs between generalism (broad adaptive capacity in diverse conditions) and specialism (high growth rates in a limited set of conditions).⁹ We question the mainline view of organizational sociology that environmental variation always favors generalism. Our study of the mortality of restaurants in 19 cities found that

⁸ The commentary charges us with blindly following the lead of population biology but also castigates us for incorporating ideas from organization theory and political sociology. We think that an advantage of our approach is its capacity to incorporate many kinds of sociological insights within its analytic structure.

⁹ Carroll's (1985) model of the effects of concentration on the life chances of specialists and generalists, which we regard as a major contribution to organizational ecology, also speaks to these issues.

variability does not inevitably favor generalists, but, as our model holds, the effect of variability depends on the "grain" of variations (Freeman and Hannan 1983, 1987a). The same conclusion holds for the semiconductor industry (Hannan and Freeman 1989).

A fifth direction of research studies the effects of competition within and between populations on rates of founding and mortality. We have formulated models of density dependence in these rates, with density conceptualized as the number of organizations in the population. We assume that processes of competition and legitimization can usefully be represented as functions of density (Hannan 1986b). Our study of national labor unions found the predicted nonmonotonic effect of density on rates of founding (Hannan and Freeman 1987) and mortality (Hannan and Freeman 1988b).

Prompted by these findings, researchers have applied the model to other populations. These include studies of exit from the American semiconductor manufacturing industry, 1945–80 (Hannan and Freeman 1989), founding and mortality of independent local telephone companies in several Iowa counties, 1900–17 (Barnett and Carroll 1987), entry into the medical diagnostic-imaging industry, 1959–86 (Mitchell 1987), foundings of voluntary social service organizations in Toronto, 1970–82 (Tucker et al. 1988), founding and mortality rates of newspaper firms in Argentina (1800–1900), Ireland (1800–1970), and the San Francisco area (1840–1975) (Carroll and Hannan, *in press*), exits from the California wine industry, 1940–85 (Delacroix, Swaminathan, and Solt, *in press*), and schisms among American Protestant denominations, 1890–1980 (Sutton, Wuthnow, and Liebman 1988).

Organizational ecology has also had an effect on applied research concerning business strategy. Brittain and Freeman (1980) examined specialism and technical innovation as dimensions describing business strategies with implications for rates of founding and mortality of semiconductor manufacturing firms. Freeman and Boeker (1984) have developed the strategic implications of population dynamics and argued that research based on such outcomes can provide an improved empirical base for research on strategy. Carroll (1984) addressed the viability of specialist strategies in newspaper publishing, brewing, music recording, and book publishing. Aldrich and Auster (1986) developed strategic implications of age dependence and size dependence in mortality rates.

The tempo of research in organizational ecology has surged in recent years (which makes it especially important to include recent work in any evaluation of either the contributions or the continued promise of the approach). This vitality makes clear that the core processes can be studied empirically with data on diverse kinds of organizations over long historic periods. Ecologists have studied the dynamics of populations of

Commentary and Debate

market organizations such as newspaper publishers and telephone companies. They have also studied such nonmarket organizations as trade associations (Aldrich and Staber 1988), religious denominations (Sutton, Wuthnow, and Liebman 1988), bar associations (Halliday, Powell, and Granfors 1987), mutual benefit life insurance societies (Lehrman 1986), and social movement organizations (McCarthy et al. 1988; Carroll and Huo 1988). The time spans covered are extremely long by the standards of organizational research—longer than a century in many cases. The feasibility of applying this model to diverse populations allows us to learn about the generality of the processes. The capacity to analyze and explain change over long periods makes it possible to build bridges with other branches of macrosociology.

Finally, what do we make of the charge that these studies could as well have been motivated by conventional sociological theory, that we have merely relabeled variables and processes? First, there is the fundamental matter of level of analysis. Whereas most organization theory characterizes processes at the level of the organization (or of the subunit), our approach works at the population level. Each level has its own distinctive processes, and neither can be reduced mechanically to the other. We think that the population focus helps explain phenomena that cannot be addressed well at the organizational level without resort to ad hoc arguments. Consider, for instance, efforts to explain the fact that some organizational forms proliferate despite individual organizations' in the populations having poor life chances (e.g., "storefront" churches and social protest organizations). From a population perspective, it is clear that such persistence can result from a high mortality rate's being balanced by a high founding rate (because of low costs of organizing and low complexity).

Moreover, the research sketched above has addressed various substantive processes that had received little or no systematic empirical attention. The change in theoretical focus led to a sharp alteration in empirical focus. This shift is most notable in studies of the effect of competition and legitimization on rates of founding and mortality. In still other instances, ecological models yield predictions that differ from those of other theories, as in the case of niche width (Freeman and Hannan 1983).

We hope that readers interested in assessing the utility of the theoretical research program of organizational ecology will consult the research cited here rather than rely on a caricature. A comprehensive treatment of our program of research can be found in our recently completed monograph, *Organizational Ecology* (Hannan and Freeman 1989).

JOHN FREEMAN
MICHAEL T. HANNAN

Cornell University

American Journal of Sociology

REFERENCES

Aldrich, Howard E., and Ellen R. Auster. 1986. "Even Dwarfs Started Small: Liabilities of Age and Size and Their Strategic Implications." Pp. 165–98 in *Research in Organizational Behavior*, edited by Barry M. Staw and L. L. Cummings. Greenwich, Conn.: JAI.

Aldrich, Howard E., and Udo Staber. 1988. "Organizing Business Interest: Patterns of Trade Association Foundings, Transformations, and Deaths." Pp. 111–26 in *Ecological Models of Organizations*, edited by Glenn R. Carroll. Cambridge, Mass.: Ballinger.

Amburgey, Terry L., Marjo-Ritta Lehtisalo, and Dawn Kelly. 1988. "Suppression and Failure in the Political Press: Government Control, Party Affiliation and Organizational Life Chances." Pp. 153–74 in *Ecological Models of Organizations*, edited by Glenn R. Carroll. Cambridge, Mass.: Ballinger.

Anderson, Philip C. 1988. "On the Nature of Technological Progress and Industrial Dynamics." Ph.D. dissertation. Columbia University, School of Business.

Barnett, William P. 1988. "The Organizational Ecology of the Early Telephone Industry: A Study of the Technological Causes of Competition and Mutualism." Ph.D. dissertation. University of California, Berkeley, School of Business Administration.

Barnett, William P., and Glenn R. Carroll. 1987. "Competition and Mutualism among Early Telephone Companies." *Administrative Science Quarterly* 30:400–21.

Brittain, Jack, and John Freeman. 1980. "Organizational Proliferation and Density Dependent Selection." Pp. 291–338 in *Organizational Life Cycles*, edited by John R. Kimberley and Robert H. Miles. San Francisco: Jossey-Bass.

Brittain, Jack, and Douglas R. Wholey. 1988. "Competition and Coexistence in Organizational Communities: Population Dynamics." Pp. 195–222 in *Ecological Models of Organizations*, edited by Glenn R. Carroll. Cambridge, Mass.: Ballinger.

Carroll, Glenn R. 1981. "Dynamics of Organizational Expansion in National Systems of Education." *American Sociological Review* 46:585–99.

—. 1983. "A Stochastic Model of Organizational Mortality: Review and Reanalysis." *Social Science Research* 12:303–29.

—. 1984. "The Specialist Strategy." Pp. 117–28 in *Strategy and Organization: A West Coast Perspective*, edited by Glenn R. Carroll and David Vogel. Boston: Pitman.

—. 1985. "Concentration and Specialization: Dynamics of Niche Width in Populations of Organizations." *American Journal of Sociology* 90:1262–83.

—. 1987. *Publish and Perish: The Organizational Ecology of Newspaper Industries*. Greenwich, Conn.: JAI.

Carroll, Glenn R., and Jacques Delacroix. 1982. "Organizational Mortality in the Newspaper Industries of Argentina and Ireland: An Ecological Approach." *Administrative Science Quarterly* 27:169–98.

Carroll, Glenn R., and Michael T. Hannan. In press. "Density Dependence in the Evolution of Populations of Newspaper Organizations." *American Sociological Review*.

Carroll, Glenn R., and Yanchung Paul Huo. 1986. "Organizational Task and Institutional Environments in Evolutionary Perspective: Findings from the Local Newspaper Industry." *American Journal of Sociology* 91:838–73.

—. 1988. "Organizational and Electoral Paradoxes of the Knights of Labor." Pp. 175–95 in *Ecological Models of Organizations*, edited by Glenn R. Carroll. Cambridge, Mass.: Ballinger.

Delacroix, Jacques, and Glenn R. Carroll. 1983. "Organizational Foundings: An Ecological Study of the Newspaper Industries of Argentina and Ireland." *Administrative Science Quarterly* 28:274–91.

Commentary and Debate

Delacroix, Jacques, Anand Swaminathan, and Michael E. Solt. In press. "Density Dependence vs. Population Dynamics: An Ecological Study of Failings in the California Wine Industry." *American Sociological Review*.

Freeman, John, and Warren Boeker. 1984. "The Ecological Analysis of Business Strategy." *California Management Review* 26:73-86.

Freeman, John, and Jack Brittain. 1977. "Union Merger Processes and Industrial Environments." *Industrial Relations* 16:173-85.

Freeman, John, Glenn R. Carroll, and Michael T. Hannan. 1983. "The Liability of Newness: Age Dependence in Organizational Death Rates." *American Sociological Review* 48:692-710.

Freeman, John, and Michael T. Hannan. 1975. "Growth and Decline Processes in Organizations." *American Sociological Review* 40:215-28.

_____. 1983. "Niche Width and the Dynamics of Organizational Populations." *American Journal of Sociology* 88:1116-45.

_____. 1987a. "The Ecology of Restaurants Revisited." *American Journal of Sociology* 92:1214-20.

_____. 1987b. "Competition and Organizational Mortality in the U.S. Semiconductor Industry. Paper presented at the eighth conference of the European Group on Organisational Studies, Antwerp.

_____. 1987c. "Specialist Strategies and Organizational Mortality in the U.S. Semiconductor Industry." Paper presented at the annual meetings of the Academy of Management, New Orleans, August 1987.

Halliday, Terence C., Michael J. Powell, and Mark W. Granfors. 1987. "Minimalist Organizations: Vital Events in State Bar Associations, 1870-1930." *American Sociological Review* 52:456-71.

Hannan, Michael T. 1980. "The Ecology of National Labor Unions: Theory and Research Design." Technical Report 1. Stanford University, Institute for Mathematical Social Sciences, Organizations Studies Section.

_____. 1986a. "Uncertainty, Diversity and Organizational Change." Pp. 73-94 in *Social and Behavioral Sciences: Discoveries over Fifty Years*, edited by Neil J. Smelser and Dean R. Gerstein. Washington, D.C.: National Academy.

_____. 1986b. "Competitive and Institutional Processes in Organizational Ecology." Technical Report 86-13. Cornell University, Department of Sociology. (To be published in *Sociological Theories in Progress*, vol. 3. Edited by Joseph Berger and Morris Zelditch, Jr. Pittsburgh: University of Pittsburgh Press.)

_____. 1988a. "Social Change, Organizational Diversity, and Individual Careers." In *Social Structures and Human Lives*, edited by Matilda White Riley. Beverly Hills, Calif.: Sage and the American Sociological Association.

_____. 1988b. "Documentation of Public-Release Data Set: National Labor Union Study." Technical Report 88-2. Cornell University, Department of Sociology.

_____. 1988c. "Age Dependence in the Mortality of National Labor Unions: Comparisons of Parametric Models." *Journal of Mathematical Sociology* 14:1-30.

_____. 1988d. "Organizational Population Dynamics and Social Change." *European Sociological Review* 4:1-15.

Hannan, Michael T., and John Freeman. 1977. "The Population Ecology of Organizations." *American Journal of Sociology* 82:929-64.

_____. 1978. "Internal Politics of Growth and Decline." Pp. 177-99 in *Environments and Organizations*, edited by Marshall Meyer and associates. San Francisco: Jossey-Bass.

_____. 1984. "Structural Inertia and Organizational Change." *American Sociological Review* 49:149-64.

_____. 1986. "Where Do Organizational Forms Come From?" *Sociological Forum* 1:50-72.

American Journal of Sociology

_____. 1987. "The Ecology of Organizational Founding: American Labor Unions, 1836-1985." *American Journal of Sociology* 92:910-43.

_____. 1988a. "Density Dependence in the Growth of Organizational Populations." Pp. 7-32 in *Ecological Models of Organizations*, edited by Glenn R. Carroll. Cambridge, Mass.: Ballinger.

_____. 1988b. "The Ecology of Organizational Mortality: American Labor Unions, 1836-1985." *American Journal of Sociology* 94:25-52.

_____. 1989. *Organizational Ecology*. Cambridge, Mass.: Harvard University Press.

Hannan, Michael T., and Nancy Brandon Tuma. 1985. "Dynamic Analysis of Qualitative Variables: Applications to Organizational Demography." Pp. 629-62 in *Measuring the Unmeasurable*, edited by Peter Nijkamp, Helga Leitner, and Neil Wrigley. Dordrecht: Nijhoff.

Hawley, Amos. 1950. *Human Ecology*. New York: Ronald.

_____. 1968. "Human Ecology." Pp. 328-37 in *International Encyclopedia of the Social Sciences*, edited by David Sills. New York: Macmillan.

Lehrman, William G. 1986. "Competing Organizational Forms in the Emergent American Life Insurance Industry: An Ecological Perspective." Paper presented at the annual meetings of the American Sociological Association, Washington, D.C.

McCarthy, John D., Mark Wolfson, David P. Baker, and Elaine Mosakowski. 1988. "The Founding of Social Movement Organizations: Local Citizens' Groups Opposing Drunken Driving." Pp. 71-84 in *Ecological Models of Organizations*, edited by Glenn R. Carroll. Cambridge, Mass.: Ballinger.

McPherson, J. Miller. 1983. "An Ecology of Affiliation." *American Sociological Review* 48:519-35.

McPherson, J. Miller, and Lynn Smith-Lovin. 1988. "A Comparative Ecology of Five Nations: Testing a Model of Competition among Voluntary Organizations." Pp. 85-110 in *Ecological Models of Organizations*, edited by Glenn R. Carroll. Cambridge, Mass.: Ballinger.

Mitchell, Will. 1987. "Dynamic Tension: Theoretical and Empirical Analyses of Entry into Emerging Industries." Paper presented at the Stanford Asilomar Conference on Organizations, Asilomar, Calif.

Nielsen, François, and Michael T. Hannan. 1977. "The Expansion of National Educational Systems: Tests of a Population Ecology Model." *American Sociological Review* 42:479-90.

Scott, W. Richard. 1987. *Organizations: Rational, Natural, and Open Systems*, 2d ed. Englewood Cliffs, N.J.: Prentice-Hall.

Singh, Jitendra, Robert J. House, and David J. Tucker. 1986. "Organizational Change and Organizational Mortality." *Administrative Science Quarterly* 31: 587-611.

Singh, Jitendra, David J. Tucker, and Robert J. House. 1986. "Organizational Legitimacy and the Liability of Newness." *Administrative Science Quarterly* 31:171-93.

Stinchcombe, Arthur L. 1965. "Social Structure and Organizations." Pp. 153-93 in *Handbook of Organizations*, edited by James G. March. Chicago: Rand McNally.

Sutton, John R., Robert Wuthnow, and Robert C. Liebman. 1988. "Organizational Foundings: Schisms in American Protestant Denominations, 1890-1980." Paper presented at the meetings of the American Sociological Association, Atlanta.

Tucker, David J., Jitendra Singh, Agnes G. Meinard, and Robert J. House. 1988. "Ecological and Institutional Sources of Change in Organizational Populations." Pp. 127-52 in *Ecological Models of Organizations*, edited by Glenn R. Carroll. Cambridge, Mass.: Ballinger.

Tuma, Nancy Brandon, and Michael T. Hannan. 1984. *Social Dynamics: Models and Methods*. New York: Academic.

Tuma, Nancy Brandon, Michael T. Hannan, and Lyle P. Groeneveld. 1979. "Dynamic Analysis of Event Histories." *American Journal of Sociology* 84:820-54.

Commentary and Debate

West, Elizabeth, and Susan Olzak. 1987. "Births and Deaths of Ethnic Newspapers in the United States." Paper presented at the annual meetings of the American Sociological Association, Chicago.

ASSESSING ORGANIZATIONAL ECOLOGY AS SOCIOLOGICAL THEORY: COMMENT ON YOUNG¹

We take issue with Young's critique of organizational ecology (*AJS* 94 [July 1988]: 1-24) and her conclusion that "if these serious problems remain unsolved after 10 years of work, I think we have the right to say the theory has had a fair showing and has not contributed to the understanding of organizations" (p. 23). We argue that her conclusion is built on three faulty propositions: (1) organizational ecology is plagued by fundamental conceptual flaws, (2) 10 years of empirical research is a "fair showing" for a theory, and (3) her critique is an accurate assessment of ecological theory. In this comment, we show that Young's critique is unsubstantiated and is based on an incomplete review of the theoretical and empirical literature.

Conceptual Issues?

Young critiques organizational ecology as biological² rather than organizational theory, arguing that "the sociological theory that extends the biological should flow from the assumptions of the biological theory or at least in some essential way be constrained or changed by it" (p. 15). Organizational ecology is not an extension of biology. It is a theoretical framework for understanding how organizational populations and communities emerge, expand, and decline (Carroll 1984; Wholey and Brittain 1986). Biology provides tools for modeling these processes and metaphors that are useful for explaining how these processes unfold over time, but it does not define the organizational processes discussed by ecological theory. Critical remarks such as "the hypothesis that death rates decline with organizational age is derived from Stinchcombe's theory, not biological theory, and indeed is contrary to the relationship of age and death rates

¹ We would like to thank Walter Powell, Lawton R. Burns, Paul Hirsch, David Torres, and Gary Wagner for their comments and John Huonker and Randall Williams for their assistance in gathering the citation count data.

² The biology Young uses is suspect. For instance, much is made of "biology's rule of interbreeding" (p. 3) in her discussion of basic ecological concepts. Since some species reproduce *asexually*, it is clear that the interbreeding rule is not sufficient to describe a taxonomy of all biological species. Young also assumes that biologists work with a completely specified taxonomy (p. 3), but taxonomic problems are common in biology (e.g., Gould 1985, pp. 78-95).

among living organisms" (p. 16) simply do not make sense if ecology is evaluated as organization theory.

By imposing a biological rather than sociological definition on ecological theory, Young severs the theory from its sociological roots. She then criticizes it for relying on "a good deal of nonbiological theory, derived from sociology, administrative science, or organizational politics" (p. 15). Organizational ecologists do not claim a status apart from organization theory; they simply argue that selection mechanisms are the primary source of organizational diversity, which makes them unique *within* organization theory (Pfeffer 1982; Scott 1987).

Young's criticisms of ecological theory unravel when the theory is evaluated in sociological rather than biological terms. One of her main criticisms, for example, is that organizational forms and niches cannot be operationalized (pp. 3–7). If organizational ecology is treated as a sociological theory, however, it can rely on information provided by participants to define both organizational forms and niches. The importance of informants is appreciated by Kangas and Risser (1979), biologists who modeled competition among fast-food restaurants (FFRs): "FFRs were placed into the multidimensional niche framework by quantifying the ways they utilize various resource dimensions. Niche utilization was then analyzed for ecological separation or isolation with a divisive classification scheme. . . . Dimensions of FFR niches include hours open, time to obtain a meal, number of parking places, and amount of inside seating. . . . Taken together, the way these dimensions are utilized by FFRs constitute niche strategies. These strategies determine the attraction for customers" (p. 145). Kangas and Risser deal with possible criticisms of their niche definition sociologically: "To address this concern, a small survey of customers was conducted. . . . Their responses verified the choice of niche dimensions studied here as being important in addition to describing several new dimensions. This illustrates one of the advantages the FFR ecologist has over the conventional plant or animal ecologist. He may converse with both the resources and the competitors to verify relevant parameters!" (p. 145). Because organizational ecology relies on the same sociological solutions, niche and population definitions are not as problematic as Young suggests.

Because she focuses on biological issues, Young misses the question that defines organizational ecology: "Why are there so many kinds of organizations?" (Hannan and Freeman 1977, p. 936). Regardless of how organizational births, deaths, and transformations are defined, the answer to this question surely involves organization creation, dissolution, and adaptation. Organizational ecologists argue that creation and dissolution are historically more prevalent than adaptation, a testable hy-

Commentary and Debate

pothesis. The precise frequencies of each may be difficult to ascertain because of disagreements over definitional boundaries, but a significant amount of research using a variety of boundary definitions indicates that selection events are far more frequent than organizational adaptations (Wholey and Brittain 1986).

Organizational ecology is not just an important paradigm in organization theory. It also deals with issues that are central to sociology. Organizational foundings and failures influence employment, social mobility, families, communities, and the content of social life. Organizational ecologists are trying to understand the processes that generate organizational foundings and failures—processes that significantly influence social structure and the social experiences of individuals.

"Fair Showing"?

We assess organizational ecology's effect on organizational research compared with that of major works in organization theory to see whether 10 years is a "fair showing," as Young assumes. We identified the comparison theoretical works by asking colleagues to list standard citations in organization theory that focus on a single unit of analysis.³ We then picked the three works with the highest average number of citations per year since publication (Etzioni 1961, 1975; Burns and Stalker 1961; Woodward 1965) and gathered data from the *Social Sciences Citation Index* on their citation histories through 1987.⁴ We also gathered information on Meyer and Rowan (1977)—published in *AJS* in the same year as Hannan and Freeman (1977)—which our survey also identified as a standard citation.

These works are used to capture the pattern of citation growth over time for important theoretical works. We use the predicted pattern of citation growth over time to assess organizational ecology's contribution to organizational research (we were not able to get information on yearly citation counts before 1966, so we do not have the first four years of data for Etzioni and Burns and Stalker). This is a particularly rigorous test

³ Prominent works such as Pfeffer and Salancik (1978), Thompson (1967), and March and Simon (1958) discuss theoretical propositions for multiple units of analysis, including individual decision makers, work groups, management coalitions, organizational subunits, etc. Since we are interested in organizational ecology's influence on organizational research, we confined our survey to other theoretical works known for contributions at a specific level of analysis.

⁴ Our survey also identified papers by Blau, Gouldner, Perrow, Child, Pfeffer, Stinchcombe, Weick, and Cohen, March, and Olsen. Since these papers are *not* cited as frequently as the comparison books charted in fig. 1, the comparison of the book citations with Hannan and Freeman (1977) is by design unfavorable to Hannan and Freeman.

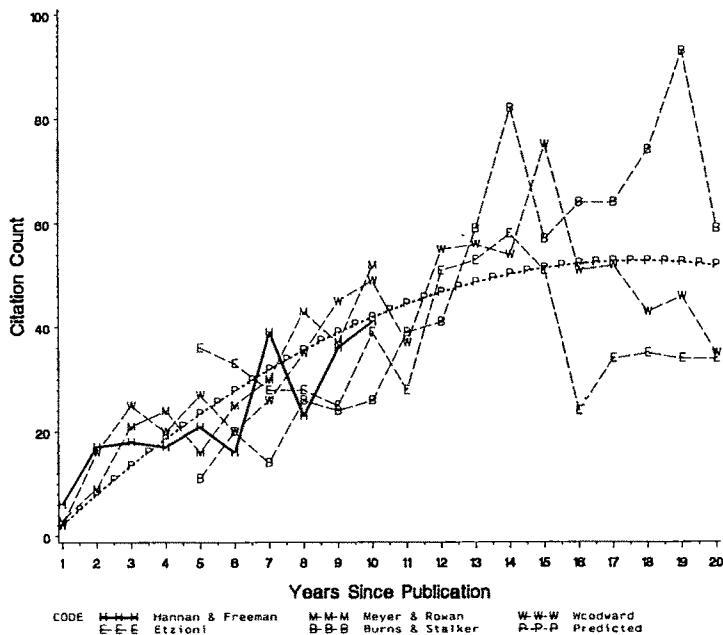


FIG. 1.—Citation counts over time. Source: *Social Sciences Citation Index*.

since it evaluates Hannan and Freeman (1977) against standard citations. The number of citations per year for each work is shown in figure 1.

The predicted citation count in figure 1 was estimated with the citation data for Etzioni (1961, 1975), Burns and Stalker (1961), Woodward (1965), and Meyer and Rowan (1977). The equation for this line is (standard errors in parentheses):

Predicted citations = $-4.18(5.37) + 6.41\ Time(.90) - 0.18\ Time^2(.03)$. The *Time* and *Time*² parameters are significant, and the equation is reasonably predictive ($R^2 = 0.52$, maximum predicted value at 17.8 years). The Hannan and Freeman (1977) citation pattern closely follows the predicted time path over its first 10 years.

The pattern of citation growth indicates that organizational ecology is just beginning to reach the point of theoretical fruition. The predicted number of citations is 240 in the first 10 years for the comparison works (the Hannan and Freeman paper has 244 citations) and 495 for years 11–20. While an evaluation of ecological theory is certainly warranted, Young's assessment of a "fair showing" is not supported. Organizational ecology is a relatively recent contribution to organization theory that continues to generate empirical research (e.g., Carroll 1988).

Is the Evaluation Comprehensive?

Balanced, comprehensive criticism plays a vital role in theory development. Comprehensive critical reviews sharpen issues, clarify concepts, and channel research efforts in a way that is beneficial to both the theory in question and the discipline. Organizational ecology certainly merits such a review, but Young's assessment is neither balanced nor comprehensive.

Researchers other than Hannan and Freeman have published works that deal with some of the concepts discussed by Young, but their work is not mentioned. If Young's intention is answering the question posed in the title of her paper, then why does she overlook McKelvey's (1982) discussion of taxonomy, Carroll's (1985) paper on competitive coexistence, McPherson's (1983) work on niches, and the Toronto group's (Singh, House, and Tucker 1986; Singh, Tucker, and House 1986) papers on entry and strategic change? These works extend the Hannan and Freeman (1977) framework and deal with many of the conceptual issues Young identifies. For instance, McKelvey (1982) discusses methods for defining organizational forms, McPherson's (1982) article describes a procedure for defining niches, Carroll (1984) discusses entry and exit definitions, and Singh, House, and Tucker (1986) study the relationship between organizational change and failure. Because Young bases her assessment of organizational ecology solely on work by Hannan and Freeman (p. 2), she mistakenly identifies conceptual gaps that are dealt with in these and other contributions to ecological theory.

Summary

The research questions raised by organizational ecology are not all answered, but the theory and research published to date have made researchers think differently about organizations and industrial communities. As a result, researchers have begun to explore how sales variation patterns affect firm labor practices, the effect of government funding on the delivery of social services, the relationship between political events and newspaper founding, the effect of competition in the wine industry, the consequences of technological breakthroughs for community structure in the semiconductor industry, the role of the state in restructuring health care, and numerous other topics in which organizations are key components of social structure. Organizational ecology raises questions about the relationship between organizations and social structure not previously considered in organizational research (Aldrich 1979; Carroll 1984), providing a bridge between organizational sociology and other areas of sociological inquiry. Whatever one thinks about these

American Journal of Sociology

contributions, it is clear that organizational ecology will continue to be a useful paradigm for the study of organizations because it deals with fundamental and empirically verifiable hypotheses.

JACK BRITTAINE
DOUGLAS R. WHOLEY

University of Arizona

REFERENCES

Aldrich, Howard E. 1979. *Organizations and Environments*. Englewood Cliffs, N.J.: Prentice-Hall.

Burns, Tom, and G. M. Stalker. 1961. *The Management of Innovation*. London: Tavistock.

Carroll, Glenn R. 1984. "Organizational Ecology." *Annual Review of Sociology* 10:71-93.

—. 1985. "Concentration and Specialization: Dynamics of Niche Width in Populations of Organizations." *American Journal of Sociology* 90:1262-83.

—. 1988. *Ecological Models of Organizations*. Cambridge, Mass.: Ballinger.

Etzioni, Amitai. 1961. *A Comparative Analysis of Complex Organizations*. New York: Free Press.

—. 1975. *A Comparative Analysis of Complex Organizations*, 2d ed. New York: Free Press.

Gould, Stephen Jay. 1985. *The Flamingo's Smile*. New York: Norton.

Hannan, Michael T., and John Freeman. 1977. "The Population Ecology of Organizations." *American Journal of Sociology* 82:929-64.

Kangas, Patrick C., and Paul G. Risser. 1979. "Species Packing in the Fast-Food Restaurant Guild." *Bulletin of the Ecological Society of America* 60:143-48.

March, James G., and Herbert A. Simon. 1958. *Organizations*. New York: Wiley.

McKelvey, Bill. 1982. *Organizational Systematics: Taxonomy, Evolution Classification*. Berkeley and Los Angeles: University of California Press.

McPherson, Miller. 1983. "An Ecology of Affiliation." *American Sociological Review* 48:519-32.

Meyer, John W., and Brian Rowan. 1977. "Institutionalized Organizations: Formal Structure as Myth and Ceremony." *American Journal of Sociology* 83:340-63.

Pfeffer, Jeffrey. 1982. *Organizations and Organization Theory*. Marshfield, Mass.: Pitman.

Pfeffer, Jeffrey, and Gerald Salancik. 1978. *The External Control of Organizations*. New York: Harper & Row.

Scott, W. Richard. 1987. *Organizations: Rational, Natural, and Open Systems*, 2d ed. Englewood Cliffs, N.J.: Prentice-Hall.

Singh, Jitendra, Robert House, and David Tucker. 1986. "Organizational Change and Organizational Mortality." *Administrative Science Quarterly* 31:587-611.

Singh, Jitendra, David Tucker, and Robert House. 1986. "Organizational Legitimacy and the Liability of Newness." *Administrative Science Quarterly* 31:171-93.

Thompson, James D. 1967. *Organizations in Action*. New York: McGraw-Hill.

Whooley, Douglas R., and Jack W. Brittain. 1986. "Organizational Ecology: Findings and Implications." *Academy of Management Review* 11:513-33.

Woodward, Joan. 1965. *Industrial Organization: Theory and Practice*. London: Oxford University Press.

Commentary and Debate

REPLY TO FREEMAN AND HANNAN AND BRITTAINE AND WHOLEY

Since the Brittain and Wholey and Hannan and Freeman comments make similar points, I shall deal with them together. Hannan and Freeman have written a new essay rather than answered many of my criticisms, which still stand and which I shall not repeat. I shall comment briefly on a few of the new points they make.

They speak of how their theory "builds on general ecological and evolutionary models of change." No one reading "The Population Ecology of Organizations" by Hannan and Freeman (1977) and noting their use of Levin's work could doubt a close relationship. I have quoted extensively from their papers to illustrate the biological roots of their work and will not repeat this exercise. Yet they say now, "We did not seek to use biological theory to explain organizational change. Nor did we propose to develop metaphors between biotic populations and organizational populations." They then state, "We relied on models from population biology because these models appeared to clarify the social processes of interest." A little later they state that they used these models as "points of departure." Brittain and Wholey seem to differ, regarding population ecology as providing metaphors. Model, metaphor, jumping-off point—which is it? They apparently cannot agree among them on which it is. More important, what difference does it make to the theory whether it is a (some) metaphor(s) or a (some) model(s)? If it is just a point of departure, why not abandon it and develop a straightforward organizational theory without reference to biology and without trying for "analogues" to definitions of species and other biological concepts? They never develop a clear line of reasoning for any of these uses of biological theory. Their discussion seems to reserve for themselves the judgment on when biological definitions and hypotheses should or should not apply.

Hannan and Freeman say that the "equilibrium assumptions usually employed by biologists and organizational analysts do not seem appropriate to analyzing change in the world of organizations." They "distrust equilibrium assumptions." They say that "organizational populations are rarely observed in equilibrium." Yet Hawley's theory, which they acknowledge as "rigorous," relied on "a principle of isomorphism" in explaining the adaptation of human communities to ecological settings: "In equilibrium 'units subjected to the same environmental conditions . . . acquire a similar form of organization.'" They say that they are extending Hawley's theory. If so, how did they avoid the problem of equilibrium?

I must take issue with their claim about the previous lack of work on samples or populations of organizations and the existence previously of

American Journal of Sociology

work only on single organizations. A casual perusal of *Sociological Abstracts* suggests this is wrong. Such study goes back at least to 1928—61 years ago—when F. Stuart Chapin published *Cultural Change*. In it, he described the process of diffusion of the city manager form of government (p. 372). There have been many comparative studies of organizations since that time by organizational sociologists, rural sociologists, and geographers.

Both sets of authors claim that I did not discuss a large literature that purports to test their hypotheses. If such a literature did not exist there would have been little point to my critique. The reason for not including it is simple. I found no adequate conceptual definition of Hannan and Freeman's major concepts. Therefore there was no way to tell whether others' measures of these concepts were valid, or whether others did or did not test their hypotheses.

I do not think that the comments on my article have advanced the cause of population ecology. I think that my previous position still holds. This theory is overdue for at least a major overhaul, and additional work is not likely to alter that situation until fundamental deficiencies are remedied.

RUTH C. YOUNG

Cornell University

REFERENCES

Chapin, F. Stuart. 1928. *Cultural Change*. New York: Century.
Hannan, Michael T., and John Freeman. 1977. "The Population Ecology of Organizations." *American Journal of Sociology* 82:929–63.

Review Essay: Radical Theory and Programmatic Thought

Politics: A Work in Constructive Social Theory. By Roberto Mangabeira Unger. Cambridge: Cambridge University Press, 1987. Vol. 1, *Social Theory: Its Situation and Tasks*. Pp. 256. \$39.50 (cloth); \$12.95 (paper). Vol. 2, *False Necessity: Anti-Necessitarian Social Theory in the Service of Radical Democracy*. Pp. 653. \$59.50 (cloth); \$18.95 (paper). Vol. 3, *Plasticity into Power: Comparative-Historical Studies on the Institutional Conditions of Economic and Military Success*. Pp. 231. \$37.50 (cloth); \$10.95 (paper).

David M. Trubek
University of Wisconsin—Madison

In the three volumes of *Politics*—the beginning of a projected series—Roberto Unger seeks to restore the relationship between social theory and radical politics. Concluding that current modes of social thought impede radical social change, Unger tries to transform the social disciplines and reorient the relationship between the study of society and the struggle to transform social life. *Politics* critiques contemporary social thought, offers an alternative mode of social analysis, reassesses key issues in world history, and outlines a program for radical transformation.

Perhaps the boldest and most controversial claim in *Politics* is the assertion that programmatic thought must be made an integral and essential aspect of all social inquiry. Unger argues that any adequate theory of society must include an effort to reimagine social arrangements. By insisting that programmatic thought is not just a by-product or “application” of social knowledge but an essential element in its production, Unger challenges the practices of most social researchers in the modern university.

Robert Mangabeira Unger teaches law and social theory at Harvard and is a political activist in his native Brazil. He has written on ethics and epistemology, legal and political theory, and the politics of Brazil. In these volumes, he has thrown down a challenge that the social disciplines can ignore only at their peril. Many will disagree with Unger, but no one can dismiss him. This is a work that demands attention and rewards close study.

I. THE RADICAL PROJECT

No idea is more important to *Politics* than the concept of a “radical project.” Unger wants to remake social thought because the existing disci-

© 1989 by The University of Chicago. All rights reserved.
0002-9602/90/9502-0008\$01.50

plines, whether Marxist, "postmodern," or positivistic in inspiration, hinder the accomplishment of "radical" aims.

Unger's idea of a radical cause draws on a "modernist" conception of the self that he developed in an earlier study entitled *Passion: An Essay on Personality* (New York: Free Press, 1984). In this conception, the personality is "an infinite imprisoned within the finite" (p. 4). The self always contains capabilities and demands disproportionate to the social and personal circumstances in which it is embedded. Yet only within these constraining contexts—modes of thought and perception, forms of personal attachment, and social and political institutions—can we realize ourselves.

Thus, Unger's concept of the self seems to rest on a paradox: there is no self outside of social contexts, but no social context exhausts the possibilities of the self. It is the resolution of this paradox that gives Unger's view of the self normative force for the social disciplines. This resolution comes from his notion of the relative "plasticity" of all social contexts, including discourses, relationships, and institutions. According to Unger, contexts are always conditional, imperfect, and transformable. The more a context is "plastic," the more easily it can be revised and, thus, the more it will permit self-realization, given the infinite possibilities and contextual nature of the self. "Radicalism," then, means the quest for more self-realizing contexts—the search for ever more revisable bodies of knowledge, personal relations, and social institutions.

Unger argues that the radical cause has its roots in Christianity and the romantic movement, leftist politics, and modern literature. While radicals of many stripes have in the past contributed to this idea of human potential, Unger thinks they have failed to carry through on or integrate their varied visions. Leftists have emphasized political and economic barriers to emancipation; modernists have focused on the constraints private life places on the development of the self. Unger wants to draw these strands together: *Politics* offers a social theory adequate for a "unified version of the radical cause" (*Social Theory*, p. 13).

II. FALSE NECESSITY

The central volume of the trilogy is called *False Necessity* because that is what is wrong with contemporary social thought and radical politics. Unger thinks our modes of understanding the world have failed to recognize fully the plasticity of society and thus the possibilities for transformation. He offers three concepts that embody the theme of liberation from false necessity: *formative contexts*, *negative capability*, and *history without a script*. Formative contexts are the institutional and imaginative practices that shape a society's routines. They are structures—like the modes of production in Marxism—that limit what can be imagined and done. Unger wants us to recognize the importance of such contexts but also grasp their mutability. No formative context is necessary or inevita-

ble; contexts can be changed in many ways and in many directions. Moreover, not all contexts are equal; some are more easily revised and thus made more likely to realize human potential.

Unger uses the term negative capability to measure the degree of revisability, or the absence of entrenched power, in any formative context. To complete the radical project, Unger tells us, we must work toward contexts with greater negative capability. These efforts must be guided by a recognition that there is no foreordained path in history. Unger rejects the idea that history has a script, that is, that the outcome of social struggle is determined by forces the contenders cannot master or restraints they cannot alter. Unger thinks most contemporary social thinkers, from Marxists to positivist social scientists, fail to grasp the mutability of contexts, the possibility of more revisable contexts, the "it could always have been otherwise" nature of historical outcomes. This failure has led the social disciplines to succumb to false necessity and to betray the radical project (see *Social Theory*, pp. 117, 223–24).

III. THE SITUATION OF SOCIAL THEORY

Unger lays out his detailed critique of the social disciplines in the first volume of *Politics*, entitled *Social Theory: Its Situation and Its Task*. While focusing on Marxism, neoclassical economics, and positivist social science, *Social Theory* critiques all forms of social theory that make the following "deep structure" assumptions: (i) we can draw clear distinctions between the frameworks (formative contexts) of society and the routines these frameworks shape; (ii) such frameworks (like "the capitalist mode of production") are indivisible and repeatable; and (iii) they must succeed each other in a predetermined sequence (e.g., capitalism must follow feudalism).

Unger argues that deep-structure theory disempowers radical politics. By insisting on inevitability and sequence, it "disorients political strategy and impoverishes programmatic thought" (*Social Theory*, p. 93). Committed to false necessity, deep-structure theory obscures the relationship between structure and agency and limits our ability to grasp transformative possibilities. To escape from these limits, Unger argues, we must rework the notion that frameworks shape social routines, removing from it any deterministic elements of the "framework" idea, and must jettison ideas of indivisibility and sequence altogether.

Politics contains an undisguised polemic against Marxism, which Unger feels is incurably wedded to deep structure and false necessity. While he recognizes antinecessitarian strands in Marx's own work and appreciates the efforts of latter-day Marxists to loosen deep-structural assumptions, Unger asserts that no amount of revision can cure this doctrine's commitment to indivisibility and necessary sequence. As a result, Marxism reifies structures, fails to recognize transformative possibilities, and cannot generate meaningful programmatic ideas.

Since Unger indictcs Marxism for reifying structure and thus paralyzing radical politics, one might expect him to applaud those who want to move away from structure altogether. But he does not. *Politics* also includes an attack on the “ulratheorists,” who are accused of not taking “structure” seriously enough (*Social Theory*, pp. 165–69). Unger sees the ultratheorists’ wholesale rejection of all ideas of structure as equally dangerous to the radical cause because ultratheorists (he has in mind Michel Foucault and some radical thinkers in the Critical Legal Studies movement) cannot produce the explanatory accounts and programmatic ideas needed for radical politics.

IV. THE ALTERNATIVE: “CONSTRUCTIVE” SOCIAL THEORY AND THE PROGRAM OF EMPOWERED DEMOCRACY

The critique of the social disciplines is a prelude to the exposition of Unger’s own explanatory theory of society, which he labels “super theory” (*False Necessity*, p. 1), and his program for social reconstruction, which he calls the “program of empowered democracy.” This program includes ideas for a radical revision of individual and collective rights, proposals for reorganizing state and economy, and a sketch of a new constitutional system.

The juxtaposition of explanatory and programmatic argument is far from accidental. Rather, Unger’s dual commitment to social explanation and to the development of relatively detailed ideas for large-scale social transformation is an essential part of what he calls “constructive social theory.” If one accepts the radical project as Unger restates it, one is forced to recognize that programmatic imagination is essential for social theory.

Unger’s account of the role of private rights in the West illustrates how the programmatic and explanatory aspects of *Politics* enrich each other. Although he recognizes the importance of the private-rights complex in maintaining the status quo in the West, Unger wants to demonstrate that the current system of private rights is not based on necessity, effectiveness, or moral superiority. He contends that our institutions of property and contract work only because they are linked with other arrangements (like hierarchical power in the workplace) that negate the liberal ideals these institutions seem to encode. He argues that, in the course of defending the private-rights order, apologists have been forced to define a series of exceptions and counterdoctrines that he calls deviant elements. He thinks that these deviant tendencies, originally introduced as justificatory moves within conventional legal discourse, would—if more fully developed—form the basis for a radical alternative to existing economic relations and institutions. In the program of empowered democracy, he develops these strands, articulating such novel concepts as a right to solidarity, grounded on reliance and trust; a right to destabilization, grounded on the necessity to revise institutional contexts constantly; and

Review Essay

a right to immunity, grounded on the individual's need for security as a precondition for participation in transformative politics.

By juxtaposing explanation and program, Unger argues, we can both better understand our situation and identify elements in the present—like the deviant tendencies in contract doctrine—that prefigure the future we aspire to. Furthermore, according to Unger, our ability to imagine credible alternatives to present contexts is essential for the radical cause of self-realization and institutional revision. We cannot revise the institutions that contain us unless we first imagine how they could be otherwise. Therefore, we *must* engage in programmatic thought or give up the radical cause altogether.

Thus the argument comes full circle. For, with the development of the program of empowered democracy, Unger completes the critique of the social disciplines launched in *Social Theory*. It is not that he disagrees with other people's programs—he thinks they have none. Unger thinks that all the main currents of modern social thought share the same failing: their inability to grasp the necessity and inevitability of programmatic thought. Marxists wait for the turn of history, positivists accept the current parameters of social life (today's formative context), as inevitable, if not also desirable, and ultratheorists engage in purely negative trashing. Unger calls on all these thinkers, in the name of the radical project many of them espouse, to turn toward programmatic thought. And he gives us a rich set of examples of what such thinking should look like.

V. CRITICAL OBSERVATIONS

Politics covers so many topics, from economic, legal, and military history to moral philosophy and political doctrine and programs and takes so many bold, controversial positions that specialists could spend lifetimes critiquing any part of the argument. It seems to me, however, there are several major themes that require close analysis if we are to accept and develop the call for constructive social theory.

The first of these is the relationship between Unger's concept of the self and his theory. Unger denies that the former is the grounding of the latter; in *Passion* he says, "We cannot hope to deduce views of the self and of society from each other" (p. 85). Yet the argument of *Politics* draws on the Christian-romantic-modernist idea of the self to such a degree that the book lacks persuasive force if one rejects this account of personality. This suggests that the ultimate effect of *Politics* depends, in no trivial sense, on Unger's ability to persuade us that his theory of the self is one we accept.

The second theme is Unger's stance toward Marxism. *Politics* includes a root and branch condemnation of Marxism. Why does he devote so much energy to an effort to condemn all varieties of Marxism to the dustbin of intellectual history? After all, Unger recognizes that recent efforts to rethink Marxism have softened, if they have not yet completely eroded, this doctrine's commitment to what he calls deep structure. Fur-

American Journal of Sociology

ther, Unger's social explanations draw heavily on Marxist-inspired work, and his program of empowered democracy includes elements drawn, *inter alia*, from contemporary Eastern European experiments. Finally, a large portion of the adherents to the radical cause profess adherence to Marxism, however diluted. Unger might have sought them as allies, but he insists they join him as converts. What explains this position?

Finally, Unger's call for programmatic thought in the social disciplines might have been more effective if he had drawn attention to and discussed the work of others who accept this view of social thought. Unger is not the first academic to believe that programmatic thought is an essential element of social theory, and other contemporary writers have developed programs with elements similar to his. More recognition of parallel trends in the social disciplines—including those in Marxist-inspired work—would have strengthened, not weakened, the arguments of *Politics*. In his analysis of legal history, Unger drew skillfully on deviationist tendencies in law to demonstrate alternative possibilities for social life; the same could have been done for a full account of the state of social theory today.

Perhaps this is the ultimate challenge for Unger and for those of us in the social disciplines who accept his views on the necessity of programmatic thought. We must all look closely at the work that is already being done and develop those aspects of the programmatic imagination already present in our fields. If *Politics* spurs such an effort, it will help realize Unger's deepest ambition, which is to reunite speculative inquiry, academic research, programmatic thought, and transformative struggle.

Book Reviews

Weber, Irrationality, and Social Order. By Alan Sica. Berkeley and Los Angeles: University of California Press, 1988. Pp. xiii + 305. \$32.50.

Charles Lemert
Wesleyan University

Weber, Irrationality, and Social Order gets under your skin. It fascinates, teaches, annoys, pleases, disappoints, excites, and more. It makes you feel as well as think. It is an excellent book. Alan Sica clearly intends the effects he creates. He wants us to think differently, better: "The goal of this book, then, becomes twofold: to stimulate Weber's theory of social action with Pareto's adroit analytic concern for the irrational; and also to open up current opinion regarding what social theory ought to take in before it hauls up the drawbridge on disciplinary limits and scholarly adventure" (p. xiii). He succeeds wonderfully in the former regard, less so in the latter.

The book's major theme is that Weber never came to terms with irrationality. The beauty of Sica's work lies in his painstaking examination of Weber's theoretical struggle with those forces that, in his mind, threaten to disrupt the rationality of human life. The idea is not new. It was developed in general terms almost 30 years ago by Alvin Gouldner in "Anti-Minotaur—The Myth of Value-Free Sociology" (an article Sica inexplicably fails to cite). It is even vaguely familiar to college students who are taught to ponder the meaning of the iron cage. What Sica does, better than anyone I know of, is give us the intellectual history of Weber's struggle.

Weber, Irrationality, and Social Order carefully examines Weber's attempts to come to grips with irrationality, beginning with the first intimations of the problem in his 1889 dissertation through *The Protestant Ethic* and his early (1903–9) methodological studies. Irrationality perplexed Weber until about 1910 but thereafter receded into the background as he turned from metatheoretical matters to the main business of his sociology, most notably *Economy and Society* (1921). Yet the problem lingered, as readers of the famous 1919 lectures on the scientific and political vocations well know.

What is the irrationality problem? At one moment, it is simply Weber's inability to resolve the essential theoretical puzzle of his life. Sica considers and reconsiders a number of explanations—some familiar, others original. One, a familiar one, is that Weber was caught in an intrapsychic

Permission to reprint a book review printed in this section may be obtained only from the author.

conflict between his mother and father that caused his breakdown and invaded his intellectual life (p. 101; cf. pp. 9–17). A second, less familiar one is that Weber was a pianist who loved music, which kept alive his contact with this deep “reservoir of irrationality” and corresponded to his “addiction” to Nietzsche (p. 109). Another is that he was caught in a Faustian dilemma in which affirmation of the certain reality of irrationality would destroy the possibility of a true science of culture (p. 153). And a fourth is that, after struggling with irrationality for nearly 20 years, Weber just gave up on solving the problem theoretically (p. 184). There are others.

Into this interpretive plenitude, Sica introduces Pareto as a theoretical foil to Weber’s confusion (pp. 225 ff.). The final chapter, in which Pareto is presented, is a marvel of careful, convincing scholarship and courageous risk taking. In the former respect, Sica addresses with consummate skill the riddle of classical theory. Did Weber and Pareto (and Durkheim) know and learn from each other’s writings? Sica’s conclusion, based on a resourceful and compelling assessment of the evidence, is that he “suspects” (p. 239) they did. But even if they did not, Pareto remains, for Sica, the perfect foil. Pareto, being the more systematic general theorist of the two, dealt with the nonrational to the very degree Weber suppressed it. Thus, Pareto lets us see what might have been had Weber not been so caught in his dilemmas (p. 261).

But the question remains, What exactly is the irrationality problem? So what that Weber could not solve it? Sica’s answer is that social theory has not accounted for the irrational or nonrational because Weber could not and his interpreters would not. If there is for Sica one explanation for Weber’s inability to deal with irrationality, it is that his commitment to science overrode his science. “My reading,” says Sica (p. 243), “. . . shows a theorist who realized twenty years before his death that nonrational components of personality (and, by reasonable extension, of societies at large) were most important to human existence, but that the project for the (Kantian) theorist included bringing these rebellious, sometimes amoral components to heel—slipping them skillfully within the realm of reason.” Subsequently, Parsons (in Sica’s opinion, the “decisive” influence on American theory) “urged, by his own example, that studying the nether world of human behavior be off limits” (pp. 22–23; cf. pp. 24, 39, 138, 142, 202). Because of this lineage of failure, social theory today is unsuited to its challenges. Hence, the conclusion that contemporary social theory has abandoned the irrational intrusion of dreams, fantasies, or the unconscious to psychological theories (p. 21; cf. p. 109). Social theory, in Sica’s view, cannot discount any aspect of social life from consideration and certainly not irrationality at a time when, some argue, life has shrunk back into those private spheres where the irrational is most aggressive and away from the public domains wherein Weber and much of classic theory believed rational behavior dominated (pp. 263–64).

But, in this second goal of the book, Sica annoys and disappoints almost in proportion to his ability to fascinate, teach, and please in his

Book Reviews

interpretations of Weber. Alexander has said ("Centrality of the Classics," in *Social Theory Today*, edited by A. Giddens and J. Turner [University of California Press, 1987], p. 46): "To defend the centrality of the classics in a strong way is to argue for an inextricable relationship between contemporary theoretical interests and investigations about the meaning of historical texts." This, surely, is a statement with which Sica would agree. Yet, when it comes to considering contemporary theoretical interests, his sense of the situation is disappointingly narrow. Irrationality is very much under discussion by feminist theorists, sociologists of the emotions, postmodernists, ethnomethodologists, and social historians of gender and race relations. Surely Goffman did not ignore it, nor does Giddens. Sica underestimates the extent to which contemporary social theory has taken on the irrationality problem because, in this book, his primary interest is the use and misuse of Weber. Believing, correctly, that Weber could not deal with irrationality, he concludes, incorrectly, that Parsons and those under his sway had so much influence on the current generation that irrationality is lost to view. There are several ways to skin a cat. Contemporary theory has drawn on many sources, classical and current (Freud, Heidegger, Nietzsche, Marx, Husserl, Schutz, and para-Parsonian readings of Weber). Even those who today take Parsons seriously identify his misreading of Weber while referring prominently to Freud and Marx.

How then, could Sica have so misjudged the need for one of his purposes? In part, I think, because of his method. To my tastes, the most entralling chapter in the book is the second, "Toward a Hermeneutic of Weber." What is required to overcome our misunderstanding of Weber is a serious reconsideration of the hermeneutics of classical thought. To this end, Sica offers (with acknowledgments to Schleiermacher, Dilthey, and Gadamer) his own list of 20 principles of a hermeneutics appropriate to reading social theoretical classics (pp. 82–86). None is particularly original. The list is not very systematic. Yet I like what he does here. For here, as elsewhere, a serious scholar is thinking for himself. Thus, for example, his twentieth hermeneutic principle is: "Interpretation is not fighting, overpowering, or dominating an object; it is subtly persuading the cultural expression to yield its fullest meaning." Straightforward statements like this set the tone for Sica's original, careful, usually convincing, always fascinating, sometimes annoying reading of Weber. Sica is willing to work within his own hermeneutic circle, and few do it as well as he. But from within even so rich a hermeneutic space, it is hard to see the landscape beyond.

Still, this is why everyone who likes good theory and admires fine scholarship should read this book. The experience is like looking over the shoulder of a very good poker player who loses a hand now and then but even in failure teaches and fascinates.

American Journal of Sociology

The Micro-Macro Link. Edited by Jeffrey C. Alexander, Bernhard Giesen, Richard Münch, and Neil J. Smelser. Berkeley and Los Angeles: University of California Press, 1987. Pp. ix + 416. \$39.95 (cloth); \$12.95 (paper).

Tobey E. Huff
Southeastern Massachusetts University

The Micro-Macro Link, a collection of 15 essays, is the product of a conference held in West Germany in June 1984. It is an intellectual feast divided into five parts: (1) general approaches to the micro-macro dichotomy, (2) rational action and macrostructure, (3) interpretive action and macrostructure, (4) affective action and macrostructure, and (5) synthetic reconstructions. By casting such a broad net, the authors manage to cover topics as diverse as methodological individualism (Boudon), exchange theory (Blau), conflict theory (Collins and Smelser), Parsonian action theory (Alexander and Münch)—including the foundations of Parsons's theory of "internationalization" in Freud's concept of the superego (Kurzweil)—a separate theory of the Freudian mechanisms of defense as a microtheory (Smelser), rational choice theory (Wippler and Lindenberg), and the "speech exchange system" (Schegloff).

A major issue is to conceptualize adequately and map theoretically the connections between "micro" and "macro" phenomena. While some of the contributors seek to establish firm divisions between the micro and the macro, the broad consensus is that the separation of the two is entirely relative. As suggested by Alexander, the idea that the micro-macro is just another name for the individual-collective division can be very misleading.

Throughout this volume there is a welcome stress on the problem of *explanation*. In chapter 1, "The Individualistic Tradition in Sociology," Raymond Boudon presents a well-developed defense of methodological individualism as a paradigm of sociological explanation. According to Boudon, any phenomenon (*M*) must be conceived to be the product of *individual* actions. These actions are a function of the situation (*S*): $M = m[s]$. The situation itself is a function of *higher-level* variables (*P*): $S = s(P)$. However, it is not clear to me what the status of these higher-level variables is and how they differ from *aggregate descriptions* of individual actions. Presumably these are "emergent" properties.

The putative contrast between collective and individual actions is the theme of James Coleman's "Microfoundations and Macrosocial Behavior." A pseudoproblem seems to be created when it is suggested that social theory is concerned with "social systems," while "empirical research . . . is largely concerned with explaining individual behavior" (p. 153), and it is further suggested that research is based on "individual" observations rather than on some imaginary "collective" observations that would presumably observe "the whole" in one scientific act. It is not clear how any

Book Reviews

"collective observation" could replace randomly selected "individual" observations that are then aggregated to serve as the *explananda*, the things to be sociologically explained.

Nevertheless, Coleman illustrates the problem of showing that a set of cultural values (or "economic preferences") are the reason for, or the "explanation" of, a general (collective) pattern of behavior by using Weber's problem of the rise of "capitalistic" behavior. But this is two problems: the first is simply accounting for the change in behavior over time (a problem in social history that traces the development and spread of a set of social doctrines or economic preferences) and the second, the problem of describing the *mechanisms* by which those new behavioral forms become "institutionalized" (something that Coleman notes we have done too little of). What is more, Coleman would like us to be able to develop "models" that specify the conditions under which particular organizational (or distributional) outcomes follow from identifiable action patterns. That is a lofty task, and Coleman's many recent essays contribute to our recognition of this sociological problematic.

One of the most fascinating papers in this volume was written by Emanuel Schegloff, a practitioner of the "new microsociology." The paper describes the speech exchange system. Schegloff suggests that studies of speech acts indicate that "turn taking" in conversation has its own organizational pattern, especially when one studies the patterns of "repair." According to Schegloff, "trouble" arises in ordinary conversation when people utter "misarticulations, malapropisms, use a 'wrong' word," and so on (p. 210). Efforts to "repair" conversational errors have a pattern that allocates priority to classes of speakers and thereby gives the whole interaction a form of social organization. Furthermore, the contextual pattern of organization, including the units of speech and patterns of action, are said to display an "extraordinary invariance across massive variations in social structural, cultural, and linguistic context" (p. 213). While this is impressive, Schegloff realizes that this very invariance might mean that such patterns are not *sociologically* relevant at all. For if something is sociologically interesting, then it must display some sort of variation according to theoretically describable social conditions. In a word, a distinguishing mark of "scientific" sociology is precisely the ability to specify the conditions under which the behavior pattern in question increases, decreases, or disappears.

The last section contains three "synthetic reconstructions" by Jeffrey Alexander, Richard Münch, and Bernhard Giesen. This section makes it clear that most of the contributors believe that there is a "symbolic" dimension to social life and that this domain is both macro and micro. Alexander's chapter attempts to reconstruct elements of various micro-theories as "analytic" elements in a more general action theory, namely, that of Parsons. Likewise, Münch provides an impressive revision of the Parsonian fourfold A-G-I-L schema. He believes that the Parsonian action theory provides a better theoretical means for integrating micro-

American Journal of Sociology

macro currents and the dynamics of interaction than any of the available schemes.

Fate and Utopia in German Sociology, 1870–1923. By Harry Liebersohn. Cambridge, Mass.: MIT Press, 1988, Pp. x + 282. \$27.50.

Thomas Burger
Southern Illinois University—Carbondale

The “founding” of German sociology, such as it was, proceeded in rather diffuse fashion, without a dominant champion or a leading school. Nevertheless, a distinctive idea appears to be reflected in the analytic perspectives of several significant early German “sociologists” the theme of the reconciliation—ever more problematic in modern society—of the demands placed on individual conduct by the objectified logic of institutional orders with the desire for a subjectively meaningful patterning of existence. The pessimistic assessment—that, with the triumph of bourgeois capitalism, complete subjugation to the imperatives of social structures has become the individual’s inevitable fate—has often been depicted as a hallmark of German sociology. Harry Liebersohn does not deny the prominence of this theme but argues that it requires amendment. Besides the determined insistence that modern society constitutes a hardened and inescapable set of constraints whose very principles of organization render all possibility of a meaningfully unified life extremely problematic, there are also “utopian” elements, that is, “imaginative visions of a political order whose realization would revolutionize existing politics and society” (p. 3).

Fate and Utopia in German Sociology, 1870–1923, is concerned mainly with five thinkers: Tönnies, Troeltsch, Weber, Simmel, and Lukács. Living in Wilhelminian Germany, all diagnosed modern society as fragmented yet powerfully dominating the individual. Each also had a vision of an authentic human existence that in one way or another was conceived as a unified life and—this attracts Liebersohn’s special attention—drew its inspiration from the eschatological tradition of Christianity. Each saw authentic existence as fundamentally threatened or rendered impossible in a society composed of institutional formations that, thwarting autonomous spiritual impulses, resisted meaningful appropriation by its members. At the same time, Liebersohn thinks, each also hoped, or clung to the belief, that the connection between the conditions of modern society and the ideal of genuinely human existence had not become entirely severed; each was actively committed to the promotion of associations whose organizational framework would be compatible with an unalienated life.

Tönnies, whose distinction between *gemeinschaft* and *gesellschaft* in classical fashion posited the contrast between a modern, inauthentic col-

Book Reviews

lective life and another, spiritually and existentially more genuine one, was a person capable of profound enthusiasm. Fueled by his disgust with Wilhelminian economic, political, aesthetic, and ethical culture, he depicted gesellschaft as a place where reason had shed its orientation to the cohesion of the whole, had become purely instrumental and preoccupied with efficiency, and thus functioned to distance individuals from each other instead of fostering mutual attachment in a direct communion of spirits. Yet, in spite of his depiction of gesellschaft as inevitable and all-embracing, he could never entirely abandon the hope for a return of gemeinschaft. While still a student, he pursued the idea of establishing a philosophers' settlement functioning, as a true home of free minds, to subvert official culture. In his last years, his yearnings culminated in a speculative projection of a new age of the spirit, the third age of history prophesied by Joachim of Fiore.

For Troeltsch, less pessimistic than Tönnies and more at ease with the modern world, the conflict between individual vitality and the dead weight of social institutions was not the specific curse of modernity but a persistent feature of all ages and types of society. While the genuine inner experience, whose prototype he took to be the religious experience, was the source of all authentic life, the maintenance of social order required that it be contained in institutions. The ongoing task, therefore, was to establish a proper balance between hardening form and dynamic spirit always ready to break out of its confines (exemplified by the church-sect dialectic). Since Troeltsch considered the well-being of this society to depend on a strong living religion, he demanded institutional reforms aimed at the creation of a rejuvenated church that fostered the expression of authentic religious feelings.

Like Troeltsch, Weber accepted modern society, but he considered the belief that a renewed Christianity would be able to give it meaning an illusion. *The Protestant Ethic*, which may be read as an allegory about Germany, portrays two ways of coming to terms with the structures of this world—both ineffective. Lutheranism could not endow them with any essential meaning for the (religious) life of the faithful; only in retreat from them could it find the place for an emotionally tinged community. Calvinism, in a unique constellation, unified the requirements of personal inner calling and external impersonal order, yet it isolated the individual. Weber never tired of underscoring the self-deception (which he tried to counter by founding the German Sociological Society as a place for value-free science) involved in any belief that science could remedy this situation. In his view, the modern individual would have to find meaning within himself, or he would find it nowhere.

Simmel, who felt relatively comfortable with modern culture, argued that its novel social forms, while threatening traditional meanings, also gave rise to new ones that individuals could make their own. Especially, he believed that the far-reaching social differentiation characteristic of modern society had created such social distance between individuals as to

American Journal of Sociology

give rise to the ideal of *Vornehmheit* (i.e., distinction or high-mindedness) as an appropriate inner attitude of the autonomous person. Persons of distinction (whom he attempted to gather in an artists' club in Berlin), turning the necessity of individualism into a virtue, would completely pattern and shape their selves. For Simmel, social life would attain an ideal state of a sort of secular salvation to the extent to which it consisted of *vornehm* persons interacting only in ways entirely compatible with their fully formed and individuated selves.

Lukács, finally, adopting a negativism reminiscent of Tönnies', found the individual to be completely trapped in bourgeois society, whose stifling conventions could contribute nothing to true individual fulfillment. Yet hidden deeply within this inhuman structure, Lukács postulated, lay the moment of an anticipated meaningful future whose idea the individual, at the height of his alienation, could intuit and embrace. He believed that, in a quasi-mystical experience, the breakthrough to the insight that the individual's complete subjugation to the structures of modern society was self-created would occur. Simultaneously with this insight would come the will to shatter the objectified conditions, to revolutionize society, and to inaugurate the community of autonomously interacting human beings.

Liebersohn's engagingly written account of his protagonists' ambivalences and shifting attitudes toward modernity, especially in its imperial German form, makes interesting reading. The reference to the religious dimension of these authors' thought is well-taken. The absence of a concluding chapter, however, is regrettable. Liebersohn might have tried, for example, to show the systematic interdependence between a theorist's utopian vision and his analytic apparatus, or to link this vision to some other facet, such as an author's politics. As it stands, the study does not culminate in a clearly argued point but merely ends.

Homo Academicus. By Pierre Bourdieu. Translated by Peter Collier. Stanford, Calif.: Stanford University Press, 1988. Pp. xxvi + 344. \$29.50.

Randall Collins
University of California, Riverside

Pierre Bourdieu, France's leading sociologist, is perhaps also the world's most successful survey researcher. If this sounds paradoxical, it is because the French intellectual world (at least in its famous part) has been dominated by philosophers and literary figures of decidedly anti-empiricist doctrine. Accordingly Bourdieu has taken to interlacing his writings with a complicated defense of objectivity. Intellectuals themselves (including his Parisian rivals) are products of the objective structures of the intellectual field, which in turn is embedded in the economic, political, and social order. Only by understanding this objective structure

Book Reviews

can intellectuals be fully honest about the sources and significance of their own claims.

Bourdieu's writings at first do not much look like what we think of as conventional survey research. Bourdieu makes little use of American-style advanced statistical methods, reports few statistical details, and pays almost no attention to how much variance is explained by his models. Nevertheless, Bourdieu's crucial intellectual resource, which has recently brought him to the top of the French intellectual world, has been that for 25 years he has directed the Centre de Sociologie Européenne and thus has had a near-monopoly on the production of high-quality survey research in France. Add to this the assistance of an extremely talented group of sociologists selected from the elite educational system (Jean-Claude Passeron, Jean-Claude Chamboredon, Luc Boltanski, Monique de Saint Martin, Yvette Delsault, and others), plus the focus of attention provided by the centralized structure of French intellectual life, and one can see how Bourdieu has had the structural location to fight his way to eminence. He has been in the position to tell the Paris intellectuals the most about themselves (which helps explain why his recent books have been best-sellers); recently he has acquired the stature to scold the "post-modernist" relativists publicly for intellectual and political irresponsibility.

At the core of most of Bourdieu's books lie his survey data. But he not only wraps them in philosophy and literary allusions; he theorizes them, displaying them not just as a report of a particular time and place but as the exemplification of a structure, as a specific manifestation of what is universally possible. To maintain his intellectual respectability in the French context, Bourdieu as an empirical researcher was not allowed the slackness of a theory/research split. His other great advantage over most American survey researchers is that he and his team spend most of their research efforts on assembling wide-ranging and often innovative empirical items. Whereas American researchers tend to be locked into the same old tired array of independent and dependent variables, Bourdieu's outfit looks for questions and sources of data covering an incredible range of the activities, ideas, perceptions, and even material objects of everyday life. This is particularly noticeable in Bourdieu's *Distinction* (Harvard University Press, 1984), but we also see it in *Homo Academicus*, where the connections, locations, and styles of judgment of French intellectuals are documented with considerable penetration.

Homo Academicus is built around a survey, carried out in 1968, of tenured Paris faculty in every discipline. The timing was fortuitous; events were building up toward the "May '68" student revolt, which polarized French academic life and ramified into trade-union strikes and a government crisis. Bourdieu presents data on the demographic backgrounds of the faculty, their schooling, social distinctions, and their power and prestige both within academic bodies and in the worlds of journalism and politics. If an American had done such a survey, it would

American Journal of Sociology

have ended up a fat nest of tables in a specialized journal of educational sociology; in Bourdieu's hands, it has had almost scandalous success with the Paris intellectual public. No doubt there was some thrill of revelation for French readers in deciphering the names of notable intellectuals, indicated only by initials, as Bourdieu presented their positions in a two-dimensional space representing the fundamental axes of the arts and social sciences faculties (American readers are given the full names). The axes represent the dimensions of prestigious/distinguished professors versus younger/less distinguished, and nonacademic forms of power versus specifically academic power.

Bourdieu's overall model is that there is a homology between external social privilege and internal dominance in the academic world. Medicine and law are traditionally the privileged disciplines, recruiting from conservative professional backgrounds and exercising administrative power. The natural sciences are academically less powerful and recruit from minority religions and modest social class origins. Within the arts and sciences specifically, classics, philology, and literature are academically most dominant, with philosophy a little less so, and the social sciences at the opposite pole; these faculties as a whole come from intermediate social backgrounds and are especially likely to owe their whole cultural capital to educational experiences (including a tendency to come from families of school teachers). Nevertheless, even here the axis of academic dominance also seems to be an axis of social conservatism.

Bourdieu takes this as sufficient for establishing his point that there is an internal structure of dominance in the academic world, and that this corresponds generally to lines of privilege and conservatism in the larger world. But then comes a structural challenge to the system. The rapid expansion of student enrollments in France—more than tripling between 1950 and 1970—produced a rapid growth in the number of faculty. The old, long-stable career rhythms were disrupted; new degree paths were instituted, causing dissension over the perceived lowering of qualifications. The upstart social sciences burgeoned, especially sociology and psychology; intellectual prestige shifted more conservatively, to intermediate disciplines like anthropology and linguistics. Worse yet, the expansion took place largely at the level of junior faculty, widening the gap between the unprivileged and the privileged in the academic world. The crisis of May 1968 was thus the result of a synchronization of several structural problems: the frustrated career aspirations of subordinate faculty, the alienation of the mass student body facing educational and social “downclassing” because of the devaluation of their credentials, and the parallel career problems of the subordinate class of intellectuals working in the Paris mass media.

Perhaps the main theoretical payoff of the book comes in the last chapter, where Bourdieu analyzes the dynamics of a mass movement made up of structurally homologous groups united by a superficial ideology. Because different components of the movement have rather different

Book Reviews

cultural dispositions and concrete problems, they mesh together best when acting in parallel rather than in close solidarity; meetings between intellectuals and workers (at the point when the student movement widened into a more general crisis) were often fiascoes. Bourdieu concludes with a critique of the illusion of spontaneity, pointing out the ways in which cultural resources dominate precisely the moments at which cultural inequality is being attacked. With this analysis, Bourcieu makes a move to shore up a gap in his earlier work: the tendency of his analysis to dwell upon the self-reproduction of a system of cultural and social domination and his relative lack of theorizing of a mechanism of structural change.

One may question, of course, whether a movement of students and young faculty provides an adequate ground on which to develop a general theory of change. The demands of May 1968 were easily bought off; Bourdieu himself has stressed the way in which subsequent structures remained formally as elitist as before although their specific contents and personnel have changed. But then, much social change does have this multilevel quality; even in the cases of far greater upheavals (such as 1789), there are many indications of how much structural continuity remained, and how much of the tumult consisted of surface rearrangements in response to deeper shifts that had already taken place. To tackle these questions requires greater historical depth and comparative scope. It appears that Bourdieu has set himself on the path to a more dynamic analysis and that we can look forward to a broader theory from him in the not-too-distant future.

Approaches to Social Theory. Edited by Siegwart Lindenberg, James S. Coleman, and Stefan Nowak. New York: Russell Sage Foundation, 1986. Pp. 398. \$29.95.

William Rogers Brubaker
Harvard University

Approaches to Social Theory contains the papers and proceedings of a conference held at the University of Chicago in November 1983. The organizers had two commendable aims: to solicit papers that would be instances, rather than mere discussions, of sociological theory and to generate fruitful exchange, among advocates of different theoretical approaches, about the central problems of sociological theory. The first aim is fully, and valuably, realized in the volume under review; the second is not.

The core of the volume comprises 11 papers, each followed by a comment and discussion, on what the editors, Siegwart Lindenberg, James S. Coleman, and Stefan Nowak, themselves acknowledge is a logically heterogeneous and somewhat baffling array of topics: the sociology of knowl-

American Journal of Sociology

edge, network theory, structural theory, purposive action theory, ecological theory, interpretive sociology, organization theory, the theory of social change, sociolinguistics, social psychology, and social movements. In several instances, the papers are curiously ill-matched to the headings under which they are subsumed. Thus Mancur Olson's rational action analysis of the closure tendencies of distributive coalitions and other groups—cobbled together from previously published material—is found under the heading of social movements, although it in fact has nothing to say about that subject. Under the equally puzzling heading of structural theory, one finds another, and better, example of the rational action approach, an imaginative paper by Anthony Oberschall that provides a transaction-cost explanation of the egalitarian and individualist character of the property rights and rudimentary governance structures established in mining camps during the California gold rush. Structural theory in the more widely accepted sense—theory that attributes causal significance to social structural properties (as distinguished from individual attributes or cultural characteristics)—is exemplified in the paper by Edward Laumann and David Knoke on issue linkage and interorganizational communication patterns in the domain of health policy. Although the authors explicitly distinguish structural theory and network analysis, characterizing the latter as a technique of data analysis not as a "theory," the paper is subsumed under the heading of network theory.

The organizational scheme of the volume derives from the original plan for a Polish-American conference, with paired Polish and American papers on each of the 11 topics. In the event, the Polish contingent was unable to attend, but the conference and the resultant volume retained the original topical structure, stranding the American contributions as singletons under each topical heading. It would have made more sense, I think, to regroup the papers. For they do, in fact, fall into two groups, with the accent on domain-independent theoretical approaches in one case, and on domain-specific substantive analyses in the other.

The first group includes a cluster of three papers employing a rational action approach. This is the only such cluster in the volume, and it reflects the editors' own perspective. Besides the papers mentioned above by Oberschall and Olson, this subgroup includes a paper by Gary Becker and Nigel Tomes outlining a formal model of the intergenerational mobility of earnings. Apart from the rational action cluster, three other papers exemplify relatively domain-independent approaches to social theory. Joseph Gusfield argues for a humanistic, social constructionist perspective, illustrating this argument with respect to the construction and reconstruction of social problems. Michael Hannan and John Freeman, in a thought-provoking contribution, apply population-ecology theory to the problem of organizational inertia, arguing that the organizational environment selects for structures that are reproducible and reliable, but just for this reason is relatively rigid. And the third is the paper by Laumann and Knoke I have already mentioned.

Book Reviews

The five remaining papers are oriented to specific substantive problem domains rather than to general domain-independent theoretical approaches. Although it is perhaps stretching the point to present them as distinctive "approaches to social theory," they are in fact the most interesting papers in the volume precisely because they reflect deep engagement with particular substantive problems. Robert Eccles and Harrison White explain the decentralization of large firms into relatively autonomous profit centers as a form of social control characteristic of large organizations, a form combining features of markets and hierarchies. Theda Skocpol and Ann Orloff compare social policies and politics in Britain and the United States during the late 19th and 20th centuries, arguing for a shift in the focus of attention from the "socially rooted demand" for welfare policies to the "political supply" of such policies, variations in which are explained by different patterns of bureaucratization and democratization. William Labov's paper serves simultaneously as an introduction to the study of socially structured and socially coded linguistic variation and as an analysis of continuing divergence between black and white speech patterns in the inner city. David Heise shows how certain basic categories of symbolic interactionism can be operationalized and used to generate formal quantitative models of symbolic interaction. And Joseph Ben-David shows how his own earlier account of the development of physiology in 19th-century Germany, an account based on the dynamics of the academic market, could be enriched by considerations he originally dismissed as irrelevant to the sociology of science, in particular, the ideological commitments of a group of midcentury "organic physicists." This nicely crafted retrospective piece, published posthumously, is a fitting farewell.

Preceding the papers are presentations from two panels, Current Issues in Social Theory and The Emergence of Sociology as a Discipline, each of which is too short (three to six pages) to be more than provocative (Siegwart Lindenberg on the imminent disappearance of sociology as a discipline and Arthur Stinchcombe on sociological scholasticism) or evocative (Immanuel Wallerstein on the 19th-century liberal-Marxist consensus and its breakup, Randall Collins on the basic health and fruitfulness of Durkheimian, Webero-Marxian, and microinteractionist traditions).

The volume concludes with a paper by James Coleman, arguing—as Coleman has argued in greater detail in these pages (*AJS* 91 [1986]: 1309–35)—that the "problem of combination" is the "central theoretical problem in sociology" (p. 347). Like the economists' problem of aggregation, this involves a theoretical move from the micro to the macro level. But the micro-macro move, Coleman suggests, is more complex in sociology, involving a variety of intervening structures other than markets. Coleman's concluding paper also usefully reviews the 11 papers, characterizing them according to their levels of analysis and types of propositions and arguing for the greater fruitfulness of those that address the micro-macro link.

American Journal of Sociology

In the introduction, the editors describe the conference as a "confrontation at the core . . . one of those rare occasions at which issues central to the discipline are joined" (p. 4). Whatever may have been true of the conference, there is regrettably little confrontation in the book. With only one paper per topic, and with a loose array of topics that is not structured around any central focus or any clear lines of opposition, there is no organized confrontation among the papers themselves. Moreover, the commentators on the papers fail, for the most part, to engage central theoretical and substantive issues (the incisive comments of Ronald Burt on Laumann and Knoke and John Padgett on Eccles and White are exceptions). Interesting and even fundamental points are raised in some of the general discussions, but the very short length and the absence of sustained exchange (were the discussion transcripts edited?) limit their usefulness. The book is valuable for its individual components, but the whole, in this case, is not greater than the sum of its parts.

Theories of Civil Violence. By James B. Rule. Berkeley and Los Angeles: University of California Press, 1988. Pp. xv + 345. \$37.50.

James W. White
University of North Carolina

Theories of Civil Violence is much more than the overview of current theories of political contention it appears initially to be. It is, rather, a case study in which the history of multiple, but selected, streams of conflict theory is used in order to study progress in social science theory generally. We have ever more data on and ever more models of civil strife; why then is there so little agreement about the theoretical achievements of the field and even the criteria of good theory, and why no clear supercession of one paradigm by another? Perhaps there is more to theoretical ascendancy and transition than the simple virtues of a theory alone, and it is this question on which James Rule focuses, using the prism of political contention.

This last is redolent of Thomas Kuhn, but this book is not a rerun of Kuhn: it is more focused, specific, and nuanced. It is focused on one theoretical area rather than on science in toto: Rule first reviews the Hobbesian stream of civil violence as individually rational and calculated and traces it down to the rational choice theorists of our day. He then addresses the diametrically opposed Marxist and Paretian traditions. Next comes the irrational, conflict-as-social-chaos, protesters-as-riff-raff school, which culminated in the theory of mass society; and after that a consideration of the collective contributions of Durkheim, Weber, Simmel, and Coser to the field. Next come Parsons and contention as value disintegration and loss of structural legitimacy; the prime example of this stream is Smelser's *Theory of Collective Behavior*. The last two substan-

Book Reviews

tive chapters are on the more politically and resource-oriented models of Charles Tilly, William Gamson, and Anthony Oberschall and on relative deprivation theory in all its variants, especially those of Ted Gurr and Edward Muller. The emergence of the former stream he sees as particularly significant: its eclipse of mass-society and collective-behavior theory in the 1960s approximates a truly Kuhnian "paradigm shift" (p. 182), and its view of contention as the rational pursuit of group interests now approaches "orthodoxy" (p. 253)—a view that even Tilly's critics would be hard put to deny.

In each instance Rule's critique centers on these questions: Can one derive from the theories in question falsifiable hypotheses? How do such hypotheses fare when confronted with data? And what is the relationship (if any—and often there appears to be none) between the falsifiability in principle of the theory, its empirical confirmation or disconfirmation, and its current scholarly acceptability? He also, however, notes that one of the reasons few models are ever discarded is that each is indeed useful in certain types of cases—it is the determination of what sorts of models fit what sorts of contentions that is so elusive.

Rule's conclusions, like his foci, are dual, concerning both social theory in general and conflict theory in particular. For theories of contention he summarizes what is generally agreed upon within the field regarding the role of governments in civil violence, the social organization of participants, the role of advance planning, and half-a-dozen other factors (chap. 8). He also presents the notion of "causal systems" (p. 235), sets of cases and contexts within which certain models are valid, arguing that there is probably no general theory of civil violence of the covering-law variety. Finally (with a nod to Mark Granovetter), he presents the notion of threshold, tipping point, or transition between compliance and defiance, order and disorder, violence and nonviolence as a particularly promising avenue for further research.

Regarding social theory in general, Rule argues that falsifiability is perhaps the best criterion of theoretical soundness and admits that almost all the theories he has reviewed do yield at least a few falsifiable statements. Moreover, he recognizes that the "intellectual ecology" of theory plays a role occasionally even more important than that of data in determining the fate of theories (p. 276). It is the "trendiness" (p. 282) of social theory and method, the consciousness of "in" and "out" theories, the short attention span of the scholarly community, the diversity of scholarly interests that obstruct the concerted, replicative attack on certain agreed-on questions and testing of certain models that might lead to a more scientifically orthodox natural selection among theories and consequently to more cumulative and comprehensive theoretical advance. This short attention span is in turn due to the scholarly reward structure, in which replication is rarely rewarded relative to "innovations" and "new departures."

As may be inferred from the above, there are adept summarizations

American Journal of Sociology

and shrewd insights aplenty, over which I have merely skimmed. For me, the concepts of causal systems and thresholds were most helpful; the summarization of more or less consensual conclusions characterizing the field today was most instructive. The enterprise is not without flaws: such a selective study necessarily leaves out many of one's own pet theorists. Indeed, sometimes it seems as if Rule's focus were not on theorists of political violence at all but on general social theorists within whose work one might discover some violence-relevant statements or implications. And the falsifiable statements derived therefrom sometimes seem arbitrary.

Overall, however, the thrust of the book is persuasive and well made. Anyone interested in metatheory, in the sociology of knowledge, in theoretical change in the social sciences, and especially in popular contention would be well advised to read it: There are guides for your own work and not a few sobering reminders of our own falsifiability. I look forward to the next volume, which deals with social theory in general.

The Statistical Analysis of Quasi-Experiments. By Christopher H. Achen. Berkeley and Los Angeles: University of California Press, 1987. Pp. xiv + 172. \$32.00.

Michael Hout
University of California, Berkeley

Most social scientists study topics that are ill-suited to experimentation. We must rely on observational studies that create serious logical and statistical problems. Twenty-five years ago, Hubert M. Blalock's *Causal Inferences from Nonexperimental Data* (Chapel Hill: University of North Carolina Press, 1964) laid down principles for observational studies using models based on ordinary least-squares regression (OLS). Experience has revealed a number of shortcomings of OLS. First there were concerns with reciprocal causation and measurement errors. More recently, concerns with problems known as selectivity and censoring have arisen (e.g., Stanley Lieberson, *Making It Count* [Berkeley and Los Angeles: University of California Press, 1985]).

Chris Achen now steps forward to make accessible to any researcher with a working knowledge of OLS both the reasons for concern and strategies for coping with the problems. In *The Statistical Analysis of Quasi-Experiments*, he lucidly describes how OLS yields inconsistent results when subjects choose their own levels of treatment ("selectivity") or absent themselves from the field of observation altogether ("censoring"). He lays out procedures for obtaining statistically consistent results by a two-step modeling strategy that incorporates the processes of selection and censoring into the analysis.

Selectivity creates problems for OLS regression because, when subjects

make important choices of interest to social scientists, they in effect voluntarily assign themselves to the "treatment group" in a naturally occurring experimental design. Usually subjects who choose "treatment" differ from subjects who choose "control group" in ways that we cannot measure. These unmeasured differences invalidate the OLS assumption that the independent variables are uncorrelated with the disturbance term. Effects that should be ascribed to the subjects' preferences, that is, to the correlation between the independent variable and disturbance, are misspecified as part of the effect of treatment on the outcome. For example, if parents pay the money for private education only if they think their children will do well in school, then the observed higher achievement of children from private schools might reflect students' unmeasured potential and not anything intrinsic to the private school treatment itself.

Achen explains both the problem of selectivity and his solution to it by introducing an econometric approach similar to the simultaneous equations approach to the reciprocal causation and measurement error problems of the 1970s. The familiarity of this approach should facilitate the diffusion of Achen's approach.

Some of the implications of selectivity are obvious; many are not. One of the most pernicious consequences of selectivity uncovered by Achen concerns the common impulse to add control variables to equations. Under some circumstances controls can make matters worse rather than better (also see *Making It Count*). Again consider the private school example. Suppose that at least one measured variable (say, social class) influences the choice between private and public schools but not achievement, while some unmeasured variable correlated with social class (say, parental assessments of prospects for success) affects both school selection and math grades. In this realistic situation faced by researchers, OLS will make it appear as though social class affected achievement while underestimating the true effect of private schools. The problem arises because at least two independent variables—social class and the type of school—are correlated with the disturbance of the achievement equation. The problem is pernicious because none of the usual statistical indicators will show any signs of trouble. In general, controlling for variables that affect selection but not the outcome itself exacerbates the inherent problems of selectivity.

Achen's discussion of censoring shows what to do when there is no control group. He cites the example of rearrests among persons awaiting trial on earlier offenses. It is very difficult to assess whether the right people are being released because the people who are held while awaiting trial have no chance to be rearrested. Because pretrial release of this sort is much less likely for people accused of serious crimes (only the "good risks" from among the people accused of serious crimes are released), the rearrest rate among serious offenders might well be lower than rearrests among petty offenders. Without a statistical control for the correlation between severity and release, the researcher will incorrectly conclude that

American Journal of Sociology

severity fails to predict rearrest or that severity has a negative effect on rearrest. Both conclusions might lead to recommendations that the authorities ought to ignore severity in deciding whom to release. The conclusions and the recommendations would be wrong.

Achen has written the successor to Blalock's *Causal Inferences* for the 1990s. He has some quirks similar to Blalock's overemphasis on partial correlations; for example, he gives far too much attention to the implausible linear probability model. But on the fundamental issues of causal inferences from nonexperimental data, this is the best book available. He starts with enough basic material to bring graduate students and rusty practitioners up to date. He bolsters this with enough rich technical appendices to inform specialists.

The Return of the Native: American Indian Political Resurgence. By Stephen Cornell. New York: Oxford University Press, 1988. Pp. viii + 278. \$29.95.

Joane Nagel
University of Kansas

In *The Return of the Native* Stephen Cornell sets about the task of understanding "the evolution of contemporary [American] Indian political resurgence" (p. 8). This important and ambitious book was motivated by its author's interest in the American Indian political activism of the 1960s and 1970s. These decades witnessed the 19-month occupation of Alcatraz Island by "Indians of All Tribes," which began in late 1969; the 1972 Trail of Broken Treaties, which led to the seizure of the Bureau of Indian Affairs offices in Washington, D.C.; Wounded Knee II, the 1973 siege on the Pine Ridge reservation in South Dakota; as well as the fish-ins, marches, demonstrations, boycotts, land seizures, and site occupations that marked the American Indian activism of the "Red Power" era.

This is dramatic and compelling terrain. Cornell charts his course from colonial and early American Indian—white economic and political relations, through the various American efforts at Indian annihilation, relocation, assimilation, and bureaucratic domination, and the various American Indian responses to these efforts. He arrives finally at the 1960s and the era of Indian activism, with its blend of tribal reservation and supratribal urban Indian perspectives, agendas, strategies, and constituencies.

Cornell's route is not direct. The book is not chronological, epochal, or topical. Rather, Cornell blends history, policy, and patterns of Indian organization and reaction to reveal the changes that have taken place among the very diverse Indian populations in the United States. Cornell then attempts to link these transformations in American Indian economic, political, social, and cultural organization to the character of Indian activism in the 1960s and 1970s.

Book Reviews

The major themes include (1) the removal of Indians to reservations and the economic and political reorganization of Indian tribes as a basis for both tribal decline and tribal renaissance, (2) the urbanization of the American Indian population (half of American Indians lived in cities in 1980) as a basis for supratrialism, and (3) the tensions generated by the rural (reservation)/urban bifurcation of the Indian population, resulting in debates over who will govern the tribes, who will run the reservations, who will speak for Indian interests.

The picture that emerges from Cornell's saga is one of Indian tribalism and supratrialism rooted in the dualism of federal Indian policy, which vacillated historically between viewing Indians as tribes on the one hand (semisovereign "domestic dependent nations" with treaties, reservations and lands held in trust, federal recognition, and various legal provisions) and viewing Indians as individual citizens on the other hand (through equal citizenship rights, urban relocation programs, public schooling, and the application of state and federal laws).

Cornell's discussion of the tribal and supratrial dimensions of Indian activism arising out of federal policy dualism and his depiction of the interaction between and consequences of tribalism and supratrialism for American Indians (chaps. 6–8) are the best sections of the book. Also notable is chapter 10, which contains the most theoretically rigorous discussion in the volume, one that brings social movement theory to bear on the question of mainly supratrial activism. Here Cornell presents an analysis of the role of organization, resources, leadership, and coalitions in Indian mobilization.

The Return of the Native asks important questions that touch on themes of historical concern to sociologists, questions that involve the conditions of group formation, the origins of patterns of mobilization, antecedents to activism, mechanisms of identity formation and transformation. Cornell draws on an impressive array of literatures and materials, incorporating diverse sources in a smooth, unobtrusive manner. The result is a book best read for a broad view and a wide scope, which does not contain a narrow theoretical agenda or the presentation of complex analyses or quantitative data.

Readers interested in specific arguments or explanations that are evaluated with elaborated historical analysis or systematically presented evidence of either a qualitative or quantitative sort will find the book less satisfying. We find here no detailed accounts of American Indian activism during the 1960s and 1970s, nor any specific discussions of the men, women, or organizations that launched these protests. Also missing is the clear designation of the issues and discontents underlying the American Indian political mobilization of the Red Power era. Nor are we provided any rigorous analysis of the debates in federal Indian policy that served as the political backdrop to Indian activism.

Cornell's book is interesting, broad, and engaging. It promises an understanding of a relatively unexamined, evocative, and both tragic and heroic province of American ethnic diversity and political mobilization.

American Journal of Sociology

Ultimately the book only partly delivers on its promise. Its arguments seem never completely laid out, its evidence seems inconclusive and often anecdotal, its index is thin, and its bibliography is "selected." These suggest a volume that attempts to appeal to both a technically inclined and an educated lay readership. The result is a highly readable book that will leave an expert audience somewhat unsated.

The Hispanic Population of the United States. By Frank D. Bean and Marta Tienda. New York: Russell Sage Foundation, 1987. Pp. xxiv + 456. \$42.50.

Richard D. Alba
State University of New York at Albany

Frank Bean and Marta Tienda's contribution to the series of monographs commissioned by the National Committee for Research on the 1980 Census, *The Hispanic Population of the United States*, provides a much needed overview of the diverse situations of Hispanic groups in the United States. In recent years, the study of racial and ethnic groups in the United States has frequently been trapped by the almost devotional study of the unique experiences of individual groups or, at the other extreme, a preoccupation with the analysis of abstract, data-driver categories—such as "Hispanics"—that cannot represent adequately the complexity of ethnic social realities. Hence, it comes as welcome relief to find a volume that takes the necessity for a comparative perspective as its point of departure.

Indeed, the diversity of Hispanic groups—and, correspondingly, the tenuousness of "Hispanic" as an analytic category—is the most salient theme of the book. In a crisply written introductory chapter, this diversity is loosely linked by the authors to the distinctive experiences of the different groups—that is, to disparities in their modes of cultural and economic incorporation. These varied backgrounds led to expectations of broad differences in the situations of the major Hispanic groups—Mexicans, Puerto Ricans, Cubans, Central/South Americans, and other Hispanics.

The heart of the book is the documentation of such differences, and this provides the reason why sociologists interested in contemporary ethnicities will want to have this book. Following what is presumably the mandate of the sponsoring committee, Bean and Tienda present these differences in copious detail—daunting, in fact, for someone who reads the book from start to finish—for a wide range of the characteristics available in census data. The book cuts a broad swath through such topics as the growth of Hispanic groups, immigration, regional distribution and residential segregation, family and household patterns, educational attainment, labor-force participation, and income. The discussions of these topics include comparisons with non-Hispanic whites (and, occasionally,

Book Reviews

to blacks) and, in appropriate places, comparisons over time for the period 1960–80. The discussions are also sensitive to the internal diversity of the various Hispanic groups, regularly identifying the different patterns for their foreign- and U.S.-born members, for instance. As basic documentation, the book includes more than 100 tables, most of them created specially for it from decennial census public use samples.

All this is to say that the book will prove a comprehensive and authoritative source for researchers who need to know what one can learn about Hispanic groups from census data. The authors are, moreover, sophisticated users of such data: the book is quite informative about the problems posed by various census attempts to identify Hispanic populations—especially for any temporal comparisons, since census enumeration procedures have been changed in each recent census.

Their adeptness at exploiting and interpreting census sources allows the authors to build up a complex, multihued portrait of how the various Hispanic groups are faring, ranging from the rapidly gained economic success of Cubans to the economic and social marginality of Puerto Ricans. Along the way, they correct numerous misimpressions and misdirected hypotheses, although they do not advance many novel interpretations of their own. For example, they offer a carefully balanced evaluation of the growth of the different Hispanic groups during the 1970s, attempting to take into account discrepancies in both 1970 and 1980 censuses. Their assessment reduces the apparent growth of these groups and—more important—results in a different understanding of the sources of growth, with greater weight attached to natural increase than to immigration.

Consider another important topic, success in school, as an illustration of the tenor of their results. Although they detect substantial gains for Hispanics when looking at the completed education of adults, they dig deeper, with an unusual analysis of current school enrollment patterns, to bring into view the auguries for educational attainment. Despite improvements over time in school attendance for all Hispanic groups, there continue to be relatively high rates of age-grade delay and school dropout in evidence for Mexicans and Puerto Ricans, the two largest among them. In a finding that the authors take pains to square with their belief that culturally targeted educational programs have value for Hispanic groups, they determine that these educational disadvantages are far better explained by parental social class than by home language environment.

Nevertheless, there are limits to the demographic approach embodied in the book. Census data are largely abstracted from the concrete social contexts in which individuals are embedded and therefore cannot inform us directly about the complex interaction between ethnic community and cultural resources, on the one hand, and opportunities and barriers present in the larger environment, on the other. Yet this interaction plays a major role in determining how groups are incorporated and what sort of life chances their members are afforded. This interaction is glimpsed

American Journal of Sociology

inferentially, to be sure, but it is impossible to devise a full account of how the members of different groups fare and why without its explicit inclusion. Likewise, the analysis suffers from the absence of any information on the subjective meaning of ethnicity. In a society like the United States, where group boundaries are permeable and ethnicity has voluntary aspects for many individuals, ethnic identity must be seen as a variable, and an important one for understanding ethnic differences in the aggregate. Indeed, it is bound up in unknown ways with the measurement of ethnicity itself, since census ethnic questions involve self-definitions as well as "objective" information possessed by respondents about their ancestry. Thus, the mandate of this book limits its theoretical horizons, but the book represents the state of the art for the quantitative analysis of ethnic groups in the United States.

From Many Strands: Ethnic and Racial Groups in Contemporary America. By Stanley Lieberson and Mary C. Waters. New York: Russell Sage Foundation, 1988. Pp xiv + 289. \$37.50.

Anthony H. Richmond
York University, Toronto

From Many Strands is one of a series of 1980 census monographs. It is not the only one addressing race and ethnic questions. Other volumes include *The Color Line and the Quality of Life in America* and *The Hispanic Population of the United States*. In this study, a systematic comparative approach is adopted and the interpretation of evidence is facilitated by the use of historical data and supplementary information derived from the 1979 Current Population Survey and other sources. The report is unique because a new open-ended question was included in the 1980 census, "What is this person's ancestry?" The instruction guide required the informant to report the "ancestry group with which the person identifies" and allowed for the possibility of multiple ancestries. The monograph begins with an evaluation of the question's validity and reliability. Stanley Lieberson and Mary Waters conclude that, as well as reflecting technical issues about the enumeration procedures, apparent inconsistencies in response patterns reflect the complex and changing character of ethnic and racial relations in the United States. Three types of "ethnic flux" in the white population are identified: commonalities imposed by the dominant society on newcomers, generational shifts and distortions in the ancestry reported following high levels of intermarriage, and an expansion of the segment of the population for whom ethnicity has little salience and who regard themselves as "unhyphenated whites."

After a descriptive account of the ethnic and racial composition of the population, including such topics as the changing patterns of immigration, the old-new distinction, and generational changes, the report exam-

Book Reviews

ines spatial patterns, cultural differences, and economic attainment. Two chapters are devoted to intermarriage. The analyses are meticulous and comprehensive in most cases, although it is surprising that the section comparing fertility trends omits the black population, although this group is included in other chapters.

By 1980, there were no significant differences in educational attainment between old and new European immigrant groups and their descendants, but black and Hispanic populations remained below average. There are some interesting residual residential and occupational concentrations, reflecting the enclaves and specialized niches established by earlier generations of European immigrants, but these are much attenuated by 1980. However, the patterns for blacks and Puerto Ricans "show both continuing discrimination and/or an inability to translate education into occupation and income at the same level as the rest of the population" (p. 155).

Rates of exogamy are of special interest as an indication of structural assimilation. The highest level of endogamy is for American-born black women (99%) and the lowest for Welsh (6.9%), but these rates must be adjusted for the size of the population concerned. The authors calculate "odds ratios" measuring the chance that a woman will marry a man of the same ethnicity relative to the probability of a woman of different ethnicity marrying a man of that group. The odds ratios range from a low of 4.5 for Irish to a high of 32,998 for blacks (table 6.1, p. 173)! When the changes over time and generations are taken into account, with age controlled, the trend is clearly toward greater exogamy and consequently a growing proportion of people with mixed ancestry. The absence of a religion question in the U.S. census makes it difficult to test the "triple melting pot" hypothesis, although, by incorporating information on religion from the NORC General Social Surveys, the authors conclude that religion does affect levels of out-marriage.

The unique ethnic, religious, and socioeconomic experience of the Jewish population is indirectly captured through special attention paid to the Russian origin group, although the authors recognize that this is not an ideal solution since survey data indicate that less than half the adult population raised as Jews reported Russian ancestry. For what they are worth, the census data confirm persisting high rates of endogamy combined with high socioeconomic status.

The monograph is an encyclopedia of information on American ethnicity that will be a benchmark for future research and later census studies, if the ancestry question is repeated. It is evident that classical views of structural and cultural assimilation and the American "melting pot" will have to be revised. A number of contradictory and countervailing forces are at work. New waves of immigration (documented and undocumented) are changing the racial and ethnic composition of the population at the same time that divisions between the "old" white ethnic groups are disappearing and those between black and white are persisting. Conflict over

American Journal of Sociology

shares in the economic and political pie are bound to go on for the foreseeable future and may even be exacerbated.

Ethnic Groups in Canada: Adaptations and Transitions. By Edward N. Herberg. Scarborough: Nelson Canada, 1989. Pp. xxii + 329. \$29.95.

Steven J. Gold
Whittier College

This book provides statistical information on selected characteristics of several Canadian ethnic groups, with data drawn largely from the Canadian census. Much of the book focuses on multiculturalism—an official policy that, since 1971, seeks to encourage the maintenance of diverse and unique ethnic communities as a basic element of Canadian society. Edward Herberg understands ethnicity as a process in constant flux: "The analysis we do is to inform on how the various groups have changed in their ethnicity" (p. 5). From this position, he is concerned with the fate of ethnic groups in an explicitly multicultural nation.

Ethnic Groups in Canada is organized in three parts. The first is entitled "Ethnic Groups and Canada." After an introduction that discusses ethnic pluralism, ethnic identity, and the social foundations of ethnic cohesion, there are chapters that describe major groups by their years of migration, extent of urban residence, and gender ratios.

Part 2 concerns ethnic-cultural maintenance in Canada. It is introduced by a theoretical chapter that builds on the work of major scholars of Canadian ethnicity—Breton, Isajiw, and Drieder. The chapters that follow report on groups' immersion in ethnic life: ethnic language maintenance, residential concentration, religious monopoly, and ethnic families' endogamy and fertility. The book's third and final part deals with the adaptation of ethnic groups in Canada, offering models and data for exploring the nature of changes that have occurred among these populations.

The book provides a useful introduction to the social characteristics of Canadian ethnic groups, and the author is to be complemented on his concise and accessible presentation of large amounts of quantitative data. He has developed means to present this information without relying on complex equations. Hence, the book is a handy reference for general readers and novice sociologists as well as those acquainted with statistical methodologies.

Herberg demonstrates that Canadian ethnic groups are markedly diverse in their behavior. No single characteristic predicts their degrees of cohesion or styles of adaptation. Summarizing several variables, he finds the greatest ethnic cohesion among Asians (except Japanese), French, Greeks, Portuguese, and Jews. Japanese, native people, and blacks—Canada's three long-present visible minority groups—have either inter-

Book Reviews

mediate or low levels of cohesion. This suggests that appearance-based similarity alone is not sufficient to yield ethnic solidarity. The least cohesive ethnic groups are the British, Germans, Dutch, Scandinavians, Poles, and Ukrainians.

While the book is strong in its presentation of data and does a fine job of showing certain temporal changes, *Ethnic Groups in Canada* is flawed by a limited theoretical outlook and exclusion of key indicators of ethnic groups' statuses and adaptation. No information is provided about the socioeconomic statuses of ethnic groups—their educational levels, occupational distributions, class memberships, per-capita incomes, welfare dependency rates, or health statuses. Despite the book's concern with "adaptation," no data on intergenerational economic mobility are offered. Consequently, readers seeking basic information about either the socioeconomic statuses of Canadian ethnic groups or the relationship between a group's political and economic characteristics and its social organization will be left unsatisfied.

Theoretically, Herberg describes ethnicity as existing solely in the expressive realm of life. Moreover, he sees ethnic identity and cohesion as something to which individuals and groups consciously and voluntarily connect themselves. The effects of structural factors such as discrimination, ethnic labor markets, immigrant economic enclaves, and coethnic exploitation receive little attention. The author does not make an explicit argument against economic interpretations of ethnic life—for example, reasserting the importance of cultural factors as opposed to structural determinants of ethnic behavior. Rather, he merely ignores them.

The author's lack of concern with the economic elements of ethnic life is surprising since, as far back as Weber's writings, and especially in the past two decades, research has shown how an ethnic group's organization, adaptation, and collectivism are strongly related to its economic activities. Therefore, *Ethnic Groups in Canada* tells only part of the story of Canadian ethnicity.

Finally, the book provides little information about the historical, cultural, or political experience of Canada's immigrants. It would be made more readable and less abstract by including case studies or in-depth examples. As it stands, the book's analytic sections posit theories of immigrant adjustment and typologies of multiethnic societies without application to real-world settings. Since the author understands ethnicity as a subjective phenomenon, the addition of some descriptive text on the making of ethnic cultures—perhaps modeled on *Beyond the Melting Pot*—would be appropriate.

As far as it goes, *Ethnic Groups in Canada* is a well-documented introduction to an important topic. Unfortunately, the author has failed to address basic indicators of Canadian ethnic groups' socioeconomic statuses. My suggestion for the author is to produce a second volume that deals with these factors in the same thorough and concise fashion offered here. With this accompanying text, readers would then have a com-

American Journal of Sociology

prehensive source of information about Canadian ethnic groups and their patterns of adaptation and cohesion.

The Overthrow of Colonial Slavery, 1776–1848. By Robin Blackburn. London: Verso, 1988. Pp. ix + 560. \$47.50.

Sidney W. Mintz
Johns Hopkins University

Robin Blackburn describes how slavery came to its bitter end, piece by piece, in the New World. He recognizes that American slave systems were integrated into what already were, as Richard Konetzke had told us, the earliest planetary empires in history. But the political order and the social order are not identical: "One of the aims of this book is to find out why the crisis in the mode of political domination sometimes detonated a crisis of the social regime, especially the institution of slavery" (p. 4). These American slaveries were unconnected to ancient forms of servitude. "The tightly coordinated labour process of the late eighteenth century sugar 'plantation' half resembled the industrial 'plant' of the future" (p. 8), Blackburn claims, as others have also suggested. But not all New World slaves worked on plantations. In a treatment analogous to Robert Padgug's, Blackburn distinguishes between the "*ancillary* slavery of early Spanish and Portuguese colonialism and the *systemic* slavery, linked to plantations and commodity production, which was dominant by the eighteenth century" (p. 9). (Blackburn's "extensive" and "intensive" slavery may mislead slightly. Such forms often coexisted and did not simply succeed each other; there is some confusing of time and form here.) The plantation's rural agrarian nature, its ability to sustain itself through the subsistence labor of the slaves, lent to it a spurious manorial quality, as Eugene Genovese, among others, has pointed out. While powering the plantation itself, the slaves often produced and even marketed much of their own food; hence they appeared to be part peasants, part proletarians, and a good deal of neither.

To qualify Eric Williams's "explanation of abolition according to which industrial capitalists did away with the slave trade and colonial slavery for essentially economic reasons" (p. 26), Blackburn builds both on David Davis, whose "impressive work focuses chiefly on the ideology of abolitionism," and on Genovese, "who explores the development of the slaves' own anti-slavery, arguing that its scope and trajectory were transformed during the epoch of bourgeois democratic revolution" (p. 27). It is Blackburn's intention to "construct a Marxist narrative of the actual liberation struggles in the different areas of the Americas . . . to establish to what extent anti-slavery, either in intention or result, transcended the bourgeois democratic or capitalist dynamic . . . [and] to acknowledge the contribution made by slaveholders to a wider bourgeois revolutionary

Book Reviews

process, to the dismantling of colonial slavery and to the birth of new slave systems." Blackburn joins the ranks of those who consider the relationships between colony and metropolis vital in understanding what happened to slavery.

After an initial discussion of the roots of antislavery, Blackburn provides an account of the winning of freedom for all the Americas. It is a sweeping and impressive achievement. To cover so much ground, even in 484 pages, does require some cramping of events, some simplification, and some slighting of sources. The contrast between intensive and extensive slavery, for instance, is sustainable mainly because of simplification, and the discussions of slave life and culture are fragmentary. Blackburn's detailed treatment is, however, entirely adequate for his larger conclusions, which are important.

Apparent conjunctures between the strengthening of industrial capitalism and the abolitionist movement do not deceive him into seeing "direct cause and effect between capitalist advance and the rise of anti-slavery . . . : such a simplified view would misconstrue the causal link and fail to account for the 'para-industrial' slavery of the epoch or for vital anti-slavery impulses that were not bourgeois in character and were even anti-capitalist. . . . Slavery was not overthrown for economic reasons but where it became politically untenable" (p. 520). But when Blackburn tells us that the "economic beneficiaries of the overthrow of colonial slavery were not industrialists but other American slave-owning planters," he may be ignoring struggles among different capitalist sectors within the same imperial system. It is true that other slaves could be forced to produce for the markets vacated by the coming of freedom elsewhere. But some capitalist sectors at home did benefit from the planters' defeats, especially when the markets for plantation products in the metropolises continued to grow.

"There are two ready-made but deceptive approaches to emancipation," writes Blackburn, "which I have sought to challenge. One of these concentrates all attention on respectable metropolitan abolitionism; the other nourishes a romantic regard for the pristine virtues of rebellion" (p. 530). By doubting both such approaches and by combining them, he gives us a view at once balanced, serious, and optimistic.

Landlords and Capitalists: The Dominant Class of Chile. By Maurice Zeitlin and Richard Earl Ratcliff. Princeton, N.J.: Princeton University Press, 1988. Pp. xxiv + 288. \$45.00 (cloth); \$12.95 (paper).

Beth Mintz
University of Vermont

This book is the culmination of a long-term project designed to investigate the structure of the modern capitalist class. *Landlords and Capi-*

talists is the continuation of the research agenda outlined in Zeitlin's now classic article "Corporate Ownership and Control: The Large Corporation and the Capitalist Class" (*AJS* 79:1073–1119). Here, in a continuation of earlier work on Chile, Zeitlin and Richard Ratcliff offer an analysis of "the concrete ensemble of social relations within an actually existing dominant class in a Western capitalist country" (p. 4).

The book is particularly interesting in several respects. By exploring the relationship between landlords and capitalists in terms of political ascendancy, it offers an analytic backdrop for understanding political transformations in Chile, including Allende's rise and fall. The authors emphasize, however, that this is a book about dominant classes of advanced capitalist societies, in general. And it is on this general level that the work confronts the literature on corporate control and intercorporate structure.

The authors begin by evaluating the status of "the separation of ownership and control" in the modern Chilean corporate system of the 1960s. Disagreeing with both the managerial conclusion that the typical large corporation is controlled by nonpropertied inside managers and the importance often attributed to relationships with other organizational actors, Zeitlin and Ratcliff find evidence of concentrated family stock ownership in the Chilean case. While most recent investigations of corporate control have assumed the organization to be the relevant unit of analysis, the authors use these findings to argue that the appropriate foci of investigation are the family networks that own and control these major companies: "Corporations and . . . banks . . . tend to be units in, and instrumentalities of, the complex intercorporate system of propertied interests controlled by principal capitalist families and their entire class" (p. 8).

Arguing that families are the fundamental units of classes, they develop their concept of the "kinecon group," a term that refers to the fusion of kinship and property systems (p. 7). Defining these groups as collections of related individuals who, when taken together, constitute the dominant proprietary interests in a corporation, the authors demonstrate that Chile's largest firms are, indeed, family controlled.

Fitting other relevant concepts into this scheme, they identify the intraclass location of the inner group—those most responsible for managing the affairs of the larger business community—finding them to be from among the richest families. A comparison of the inner group and finance capitalists reveals that Chile's largest banks and nonfinancial corporations are led by members of the most important capitalist families, those whose interests cover financial, commercial, and industrial enterprises. Finance capitalists and the inner group, they conclude, are "all but synonymous" (p. 139). Moreover, the structural connections between landlords and capitalists again reveal an internally differentiated, yet cohesive, dominant class.

The portrait that emerges is one of a central core of intermarrying

Book Reviews

families in which banking, industrial, and landowning interests coalesce. Rather than support the idea of conflict between feudal and capitalist modes of production, Zeitlin and Ratcliff describe the merger of agrarian and corporate interests in which the landed capitalist family represents the generalized interests of their class.

While some of these ideas will be familiar, what should be well received by those interested in kinship analysis of the capitalist class are the empirical evidence and the analytic procedure. The authors demonstrate that exploring the kinship networks of capitalists produces important missing information about both capitalist unity and intercorporate control. They demonstrate both the feasibility of this type of research and its importance.

In addition, they provide a set of research questions and procedures, "a paradigm for investigating corporate control" (p. 55), that can be used as a rough guide for this type of research. While they warn us that the project is incomplete and that the size of the task is overwhelming, this is an implicit invitation to explore the United States. And although they invite us to do so with trepidation, they are convincing about the necessity of such an analysis.

The work does leave ample opportunity for refining the study of family ownership ties. In addition to the typical problems found in kinship analysis, we find many of the difficulties of more general network applications. What, for example, is the best way to define a cluster of intermarried families? Should clusters be mutually exclusive? What is the maximum number of links defining a relationship?

While these questions remain for future research, the present book offers a concrete illustration of the limitations of contemporary investigations of corporate control. Their systematic analysis of intraclass locations reminds us of the importance of class in corporate affairs, and this reminder is extremely well taken.

Social Mobility and Class Structure in Modern Britain, 2d ed. By John H. Goldthorpe, in collaboration with Catriona Llewellyn and Clive Payne. Oxford: Oxford University Press, 1987. Pp. ix + 377. \$39.95 (cloth); \$19.95 (paper).

David B. Grusky
Stanford University

In the first edition of *Social Mobility and Class Structure in Modern Britain*, John Goldthorpe and his collaborators returned to some of the long-standing debates over the structure of social mobility, but they did so armed with models that make a conceptual distinction between the

underlying pattern of "social fluidity" and the mobility rates directly observable in a cross-classified array. The authors used these models to describe the densities of class inheritance and exchange in Britain; moreover, they discussed the consequences of social mobility for kinship ties, and they described the implications of recent patterns of class recruitment for the formation of class-based collectivities. In the current edition, these original analyses are left largely intact, but a supplementary section on "issues of the present day" has now been appended to the end of the monograph. This section includes chapters on the class mobility of women, the cross-national contours of mobility, and the structure of recent trends in the British stratification system. These chapters are no disappointment; they are as brilliantly argued as the original ones.

The most controversial chapter will no doubt be the one addressing the class mobility of women. It is here that Goldthorpe and Payne defend the "conventional approach" to locating women within the class structure of industrial societies (pp. 281-85). According to this viewpoint, the family is the elemental unit of stratification, and the occupation and employment status of the family head defines the class position of all its members. It should be emphasized that the conventional approach does not exempt scholars from studying women; instead, as noted by Goldthorpe and Payne, the logic of this position requires investigators to consider the mobility that women experience as a result of *marrying* into their conjugal family. It follows that a study focusing exclusively on the interclass mobility of men could in theory produce a misleading picture of the overall fluidity in the stratification system. This possibility was not borne out; indeed, Goldthorpe and Payne find a surprising similarity between patterns of male and female mobility, and they conclude from this that the male-centered analyses of the past may not have led us far astray. In their final comments, they even suggest that the standard male-centered analyses may well be an "optimal strategy" (p. 296), since the limited resources for surveys of this kind are best concentrated on a single subpopulation. It should be clear, of course, that this conclusion stands or falls on the assumption that married women derive their class identities from the occupations and employment statuses of their husbands. This assumption may suffice in an approximate fashion over the period covered by the British survey, but given the current trends of change it may be hazardous indeed to adopt the same view in future efforts.

The next chapter places the results from the British survey in comparative perspective. The empirical basis for this chapter is a new cross-national archive of mobility data. Here, Goldthorpe highlights the distinctive features of the British mobility regime, and he demonstrates that these can be largely attributed to the early industrialization of Britain and the special rhythm and timing of its occupational transformations. In fact, once the cross-national variations in class distributions are controlled, the distinctive features of the British case disappear almost entirely. The mobility regime becomes distinctively *average* in its contours

Book Reviews

and densities; it assumes an "intermediate position" around which the regimes of other industrial countries deviate to a greater or lesser degree. The implication of these results, according to Goldthorpe, is that Britain may not be burdened with the "institutional peculiarities" assumed by some commentators (p. 323). This opens up the possibility of egalitarian reforms in Britain; indeed, in a country that occupies an "intermediate position" among its peers, there may be some latitude to change the stratification system through political interventions of various kinds (p. 323).

The monograph closes with a discussion of the future of the British stratification system. Although the overall level of social fluidity in Britain has remained stable throughout the postwar period, Goldthorpe rejects the view that this reflects an underlying "homeostatic mechanism" in the mobility regime. Instead, he argues that class-based inequalities of opportunity *can* be narrowed, but only if the working class can push through various egalitarian reforms. It is relevant, then, that the prospects for a revived Labour Party are not altogether poor.

It may be useful, in evaluating this viewpoint, to keep in mind that opportunities for mobility in the United States and other countries have been gradually increasing over the postwar decades. In some cases, these trends might be attributed to egalitarian reforms, but we cannot dismiss the possibility that "endogenous processes" associated with industrialization are also playing a role. If we wish to assess these types of competing arguments in a convincing way, we will have to move beyond the bivariate mobility table and estimate more comprehensive models of the stratification process. However, until models of this kind are constructed, the arguments advanced by Goldthorpe will clearly stand as the starting point for all future efforts in the field.

State and Revolution in Finland. By Risto Alapuro. Berkeley and Los Angeles: University of California Press, 1988. Pp. xiv + 315. \$30.00.

John D. Stephens
Northwestern University

Why should you read a book about a small country that, unlike Sweden with its model welfare state or Korea with its recent history of phenomenally rapid, egalitarian development, has no apparent characteristic that makes it a special case worthy of general attention? Indeed, there is a chance that this superb study of Finnish political development will be read only by Scandinavian specialists. This would be unfortunate for two reasons. First, as Risto Alapuro shows, Finland has specific characteristics making it of special interest to comparativists. The showing of the Finnish Social Democrats in the 1916 elections (103 of 200 seats and 46%

of the vote) was clearly the strongest of any socialist party (or parties) in any country at the time and (to my knowledge) was surpassed only by the Swedish Left (Social Democrats and Communists) two decades later. Yet over 70% of the population was still employed in primary sector activity. Moreover, less than two years later, this powerful movement attempted to carry out a "defensive revolution," which ended in defeat in a civil war. The two sides were cut almost entirely along class lines pitting the urban and rural proletariat against the bourgeoisie, peasantry, and middle strata, thus making it perhaps the clearest example of Marxian class warfare to date.

Second, *State and Revolution in Finland* is theoretically and methodologically important. Theoretically, Alapuro draws on Barrington Moore; Tilly's work on mobilization, state building, and revolution; Rokkan's contributions on state and nation building, social cleavages, and party system formation; and Skocpol's insights on role of the state and the interstate system to develop his own approach. The final product is not an ad hoc use of these authors but rather a synthesis that parallels other recent contributions to the study of long-term macrosocial change. Methodologically, the book is a masterly application of the "case in comparative perspective" method. In addition to his explanation of Finnish development, he strengthens his argument with highly controlled comparisons with other East European and Scandinavian countries.

After an initial chapter laying out the problem, three chapters are devoted to an analysis of the making of the Finnish state in the 19th century and the development of the class structure. The next three analyze the development of Finnish nationalism and political development up to the elections of 1916. In these sections, Alapuro emphasizes the "Scandinavian" nature of the Finnish class structure, that is, the strength and political autonomy of the independent landholding peasantry and the corresponding weakness of a large landed class and thus of "labor-repressive agriculture" in Moore's sense. The dominant class in the country was an urban (and in part Swedish-speaking) upper class that derived its power from control of the bureaucracy. However, because of the timing of the demographic transformation and simultaneous development of rural based industry and capitalist transformation of the countryside, Finland contrasted with the rest of Scandinavia in that it was characterized by the simultaneous development of an industrial and an agricultural proletariat. These groups formed the basis of social democracy, and Alapuro argues that this development pattern does much to explain the spectacular performance of Finnish social democracy in the 1916 elections. Although Finland was a grand duchy of Russia, the Russian role was mostly limited to supplying the ultimate source of coercion. Thus, the development of a modern state apparatus proceeded relatively unimpeded by the imperial power. Compared with the rest of Eastern Europe, the formation of a national identity developed rather smoothly. Here, the articulation of the class and ethnic structures was important: there was no

Book Reviews

Russian upper class to speak of, and the Swedish upper class was not involved in the coercive exploitation of agricultural labor. Thus, the development of Finnish nationalism was not such a great threat to them, and the nationalist movement was aided rather than opposed by economic, political, and bureaucratic elites.

The sections on Finland culminate in three chapters covering the abortive revolution of 1917–18, including the subsequent development of the quasi-fascist Lapua movement. Alapuro argues that the revolution was essentially a defensive action on the part of social democracy and the Red Guards, the socialist militia. The first critical event was the February Revolution in Russia, which led to the crumbling of the state's means of coercion in Finland. Into this vacuum stepped the workers militias and then in reaction bourgeois militias set up by the parties of the Right. The second critical event was the unilateral declaration of full internal self-government by the Social Democratic led government. With the support of the bourgeois parties in Finland, the Russian provisional government rejected this move and countered by dissolving the government and calling new elections, which the bourgeois parties won. The Social Democrats did not accept the legality of the new government. Both sides continued to strengthen their militias, with the bourgeois government finally achieving the upper hand. In January 1918, the government officially declared the bourgeois militia to be the troops of the government, touching off the civil war. The Whites' victory in the civil war was followed by a reign of terror in which thousands of Red supporters perished. In sum, it was ultimately the breakdown of the state's coercive capacity that pushed the reformist Social Democrats into an abortive "defensive revolution."

The comparative analysis is concentrated in the final two chapters. In the penultimate chapter, Alapuro compares the Finnish developments with contemporaneous revolutions and upheavals in other polities in Eastern Europe, which like Finland had been formed from the ruins of the 19th-century empires. However, they differed from Finland in that the countryside was covered with large estates dependent on labor-repressive agriculture. Alapuro makes a very convincing argument that this resulted in a more radical turn in which the Bolsheviks became the dominant political force and carried out a revolution, which was defeated only because of German and later entente intervention. The final chapter is devoted to a wide-ranging, somewhat less focused, but nonetheless very insightful comparison of political development in Finland with development elsewhere in Scandinavia and Eastern Europe. Here, Alapuro shows how differences in class structure, state structure, and interstate relations and their interrelation resulted in the distinctive path followed by Finland.

In sum, I very strongly recommend this fine book to all comparative social scientists interested in European political development, revolutions, state building, or nationalism.

American Journal of Sociology

Evangelicalism: The Coming Generation. By James Davison Hunter. Chicago: University of Chicago Press, 1987. Pp. xi + 302. \$19.95.

Terence C. Halliday

American Bar Foundation and University of Chicago

"One can only be amazed," concludes James Davison Hunter, "by the resilience of Protestant orthodoxy in its long encounter with the modern world order." Once a discredited loser at the 1925 Scopes trial, the modern evangelical movement has fought back from 20th-century obscurity (although without ever recapturing its 19th-century hegemony) to constitute some 22% of Americans, to grow at twice the rate of other Protestant denominations, to give 44% more than liberal Protestants, to enroll approximately 2.5 million students in its primary and secondary schools, and to expand prodigiously its publishing and media enterprises. Evangelicalism's emergence as a political force has projected all this into the forefront of the national consciousness.

Hunter's concern in *Evangelicalism: The Coming Generation*, however, is the fate of orthodox meaning. Relying principally on a survey of evangelical college and seminary students—who he argues are the pool from which a future evangelical leadership will be recruited—Hunter looks, with varying success, at cultural trends in four areas: theology, work, family, and politics.

Beginning with theology, he notes that while the "coming generation holds the Bible in high regard," there are significant concessions to once disavowed biblical criticism, a wary openness to hermeneutical subjectivism, and a more positive valuation of neo-orthodox and especially Barthian theology. Moreover, social responsibility has recaptured some of the legitimacy it had in 18th-century British and 19th-century American Evangelicalism. Ethical boundaries have shifted, and the coming generation has immersed itself in general American postwar culture. Hunter maintains that the evangelical family is sentimentalized and bourgeois. Its normative expectations have become blurred, and there is a drift towards "androgynous" role definitions of husbands and wives.

Politics constitute the most visible side of conservative Protestantism. Surprisingly, a majority of Evangelicals are either neutral or opposed to the Moral Majority. Yet Hunter argues that it is in the private sphere—abortion, rights of women, and homosexuality—where modernity and conservative Protestant religion will clash most vigorously in the political arena. Ironically, the political civility and tolerance that Hunter finds in this new generation may subvert Evangelicalism; the norms of good taste and tolerance may weaken the strength of orthodox Protestants' privately held views and the readiness to proclaim them publicly. Hence Evangelicalism may become something less than evangelistic.

These cultural trends have at once brought Evangelicals more thoroughly into the mainstream of American life and made its cultural integrity more suspect. An analysis of Hunter's metaphorical undercurrent

Book Reviews

reflects his moderately pessimistic prognostication. Assumptions about the evangelical self are under "assault"; moral boundaries have "weakened"; the traditional family may need "salvaging"; the politics of the private sphere could be the "final battleground"; symbolic boundaries of orthodoxy have "eroded"; the cultural tradition is in "disarray."

This religious community therefore confronts modernity by being caught in the tension between accommodation, which would buy cultural acceptability at the price of religious fidelity, or rejection, which would preserve orthodoxy at the cost of social marginality. Ultimately, Hunter hedges. Although his poetics are unrelievedly forlorn in their vision of a distinctive evangelical future, his analysis is more cautious. He hints at radical transformation without dogmatically proclaiming it.

Hunter has understanding, insight, and a strong thesis. Nevertheless, for all the value of stimulating a debate within and about Evangelicalism, methodological problems pose real questions about Hunter's target population. The most crippling is the exclusion from this study of all those evangelical college students in secular universities whose faith is tended by parachurch groups. The very students he excludes are likely to be better educated, to have more cultural authority, *and* to evince a more robust faith, according to Hunter's own logic, because they must sustain it in a somewhat hostile setting. A larger methodological question comes from Hunter's interpretation of long-term historical change. The theme recurs that Evangelicals may be following—with a half-century lag—the assimilation to modernity that marked religious liberalism. But, Evangelicalism *now* differs from an earlier liberalism in one critical respect: the example of what happened to the latter is manifest for the former. Cultural movements can learn.

In the final analysis, Hunter's data can be given an alternative interpretation, which he broaches but ultimately rejects. For some decades it has become clear that aspects of Evangelicalism's theology, ethics, and politics were ultimately untenable. Hunter sees current changes as drift down an assimilationist slope to cultural capitulation, but they might equally be viewed as cultural repositioning—less the erosion of cultural boundaries and more their shift to defensible ground. The family is an interesting case in point: at once, Evangelicals have managed to articulate a fairly clear conception of a Christian family (whether "bourgeois" or not), they have grounded much of it biblically or theologically, and they have appropriated to their cause the benefits of modern family counseling. Is this decline or revitalization?

Nonetheless, Hunter's searching and thoughtful explorations of conservative identity in part 3 of the book will allow no false security on the part of Evangelicals. Bases of belief have shifted, many symbolic boundaries are unclear, and, most of all, civility represents an enormous challenge to orthodox beliefs and a distinctive way of life. Neither observers of religion nor leaders of Evangelicalism can afford not to read this book and grapple with its arguments.

American Journal of Sociology

Putting Asunder: A History of Divorce in Western Society. By Roderick Phillips. Cambridge: Cambridge University Press, 1988. Pp. xx+672. \$39.50.

Douglas L. Anderton

University of Massachusetts at Amherst

As others have, Roderick Phillips sets out to explain the breakdown of the traditional family. The breadth of temporal and geographical coverage, and a more narrow focus upon divorce, distinguish *Putting Asunder*. The first two chapters present the background of Western divorce as revealed in early Catholic and Reformation doctrines. Phillips argues the disposition of the reformers toward divorce reflected an increased valuation of marriage as a working institution. Earlier Catholicism considered marriage a remedy to fornication, inferior to celibacy, and defended the marital union in face of its practical dissolution.

Phillips proceeds to a triad of divorce histories in 17th-century England, colonial America, and 18th-century France (chaps. 3–5). England proved an exception among Reformation states in failing to liberalize divorce. The colonies liberalized divorce after the American Revolution and their break from antidissolutionist English authority. Finally, France provides a case of dramatic divorce liberalization, enacted in 1792 but then overturned by the Bourbon Restoration. The role of law and the state is evident in all three settings. The following three chapters present brief accounts of the secularization, legislation, and alternatives of marital behavior during this period (chaps. 6–8).

Chapters 9 and 10 form the nucleus of the text, addressing the meaning and social context of marriage breakdown in the past. Phillips argues marriages in the past were more stable because of flexible marital expectations. With lower expectations, marital partners accommodated greater marital violence and double standards for behavior. The harsh realities of familial economies and the use of force (e.g., domestic violence, witchcraft persecution) effectively punished marital breakdown and reinforced limited expectations.

The remainder of the text concerns contemporary divorce. Chapters 11 and 12 trace the rise of divorce legislation and related social movements over the 19th century. Legislation and social movements represented largely conservative concerns for the traditional family. Social movements, Phillips notes, neither deterred nor effectively promoted divorce. The two final chapters consider mass divorce in the 20th century, focusing on the two world wars, the Depression, and the past three decades. This discussion returns to earlier arguments about the causes of rising divorce, and Phillips concludes that marriages have indeed become less stable as alternatives to marriage have become more feasible and marital expectations have increased.

Weak uses of evidence and vague causal arguments lead to the major

Book Reviews

difficulties in Phillips's text. In the discussion of early doctrines and legislation, evidence of correspondence with common behavior is marginal. Geographical portraits of the 17th and 18th centuries lack careful causal argument. No convincing case is given for the English exception among Reformation states. Asserting that American liberalization of divorce arose from the escape of English influence is a speculation that ignores dramatic changes in social composition and selective mass migrations preceding the Revolution. The French case is more carefully presented. Through all three cases the common thread of state influence is apparent, yet not directly elaborated.

Although it is difficult to disentangle the tumultuous changes of the 18th century, little insight, or speculation, is provided about the specific processes of secularization in marital behaviors. Again, evidence is weak as Phillips employs divorce petitions without population denominators and percentages without respect for the number of cases represented. The discussion of divorce alternatives is entertaining but lacks evidence on the frequency of such behaviors. Where present, evidence is often disassociated from theoretical discussions. Evidence supporting the urban prevalence of divorce, for example, is remote from discussions of secularization.

The central theoretical discussion (chaps. 9 and 10) is loosely crafted. Phillips cites largely discredited assertions of an 18th-century sexual revolution as exemplary. He argues that low expectations accommodated marital discord and violence, thereby accounting for stable marriages. The terms "marital expectations" and "stable marriage" are employed in unfortunate dissonance with evidence. First, the causal force of individual expectations is weakened by Phillips's own demonstration that those with differing expectations could not reasonably behave otherwise in the social context. Second, considering such marriages more stable is a potentially misleading interpretation. Finally, separating the discussion of domestic and social contexts across chapters treats the domestic unit, rather than women, as the object of social control (betrayed by Phillips's repetitive use of the corrective phrase "especially women").

Discussion of contemporary divorce is weakly linked to preceding arguments. Changing expectations are not directly approached as causal influences. Individual level reasons for divorce that might reveal changing expectations of marital rights are absent. Again, evidence is poorly presented, as when crude divorce rates (which fail to control for dramatic changes in marriage surrounding the war years) are employed.

Despite these criticisms, the text can be recommended for its history of social doctrines. Ultimately, however, causal arguments and supporting evidence are weak. Perhaps an explanation should have been more directly sought for the enforced stability of marriage in the past. Considering the rise in divorce as marital breakdown versus a recognition of rights simply reflects whether one is looking at the present from the patriarchal past or looking back from the present.

American Journal of Sociology

Uneasy Virtue: The Politics of Prostitution and the American Reform Tradition. By Barbara Meil Hobson. New York: Basic Books, 1988. Pp. vii + 275. \$20.95.

Louise A. Tilly
New School for Social Research

Barbara Meil Hobson has adopted a complex design for *Uneasy Virtue*, her study of prostitution and reform, past and present. At the heart of the book is a historical case study of the politics and policing of prostitution in Boston from 1820 to the present. In addition, she compares the Boston experience with that of other American and European cities of various times. Finally, Hobson also traces the actors—prostitutes, politicians, reformers, experts—who participated in this sweep of history on the local, national, and international levels. Throughout, she demonstrates clearly “how essentially contested prostitution policy has been over the last century and a half, and . . . [that] the differences go to the heart not only of a society’s organization of class and gender but also of the state’s role in regulating morals and markets” (p. 3). Some themes recur, however: U.S. policy probably was unique in the tension between intervention on moral grounds and free-market principles; women’s limited job possibilities and poor wages were, and are, the “bitter root” of the prostitution economy; European registration and reform programs are embedded in national history and cultural context and very likely cannot be simply replicated here; prostitution “can never empower women or provide them with economic autonomy” (p. ix).

In 1823, after a period of urban growth and middle-class religious agitation, Boston’s mayor, Josiah Quincy, launched a campaign to repress prostitution. New institutions for incarcerating prostitutes were established, and crowds attacked brothels as city dwellers united to protect their real estate. Law enforcers pursued persons accused of public chastity offenses, overwhelmingly lower-class young women, not their customers. Sex commerce was pushed to the outskirts of the city. Another wave of repression in Boston occurred in the 1850s as a larger-scale, more institutionalized prostitution trade emerged. There ensued a period of relaxed control (but not toleration).

Those who supported reform on the local level included middle-class women who believed that prostitution expressed class and gender inequalities; for them, the problem was male dominance and the double standard. The answer was controlling male sexual behavior and protecting women. In the Progressive Era, similar moral outrage against the by-then even larger, still more rationalized business of prostitution was combined with an acute consciousness of the very real economic and social problems women faced in the labor market and in corrupt city police and court systems. New laws and administrative procedures failed to eradicate prostitution’s evils, however. World War I brought a “witch hunt” of

Book Reviews

prostitutes, fueled by fear of venereal disease, that denied these women their elementary civil rights.

From the 1920s on, sociological, psychological, and criminological experts proposed new analyses of deviance. W. I. Thomas's *The Unadjusted Girl* (1923) was broad and generous; he saw the prostitute not as doomed by her defective personality but as the product of a tension between personality, values, and lack of social support. Other experts were more biased and punishing in their interpretations.

Hobson closes with an examination of the current situation. Feminists in the United States have not developed a single position on prostitution. Most agree that the double sexual standard should be abolished and that sexual privacy is an individual's right. Beyond that, some see prostitution as a way to economic independence and a means for women to control their own sexuality, while others see it as sexual slavery. No mass mobilization and movement like those about abortion and rape have developed. This contrasts with the situation in Sweden, where socialist feminists have successfully sponsored legislation that punishes those who profit from the business of prostitution, not the prostitutes or their customers; establishes social work support systems; and promotes education about equality between the sexes. Nevertheless, Hobson concludes, Sweden's feminists have not had to deal with either a right-wing moralistic crusade or a "prostitution as work" position in their midst, as have American feminists supporting similar programs.

There are no easy answers in this book. The virtue of Hobson's study lies in its demonstration of the links between the prostitution economy, gender ideology, and labor markets. In delineating change in ideologies about sexual behavior and temporal and national variation in political approaches, moreover, she suggests the possibility of constructing more equitable political, economic, and sexual relationships.

Demographic Behavior in the Past: A Study of Fourteen German Village Populations in the Eighteenth and Nineteenth Centuries. By John E. Knodel. Cambridge: Cambridge University Press, 1988. Pp. 616. \$59.50.

Susan Cotts Watkins
University of Pennsylvania

The expectations of demographers about this book will be high, and they will not be disappointed. The meticulous attention to the quality of the data and the analytic rigor demonstrated in John Knodel's previous work on Germany and Thailand are again evident. *Demographic Behavior in the Past* covers the major aspects of demographic behavior—mortality, marriage, fertility (migration is not covered)—and their interrelationships. The data are a set of 14 German village genealogies, and most analyses concern couples who married between 1700 and 1899. This

period encompasses the shift from natural fertility, in which the flow of children within marriage was essentially unregulated, to the adoption by married couples of deliberate fertility regulation.

Historical demographers will find the book extremely valuable. Many issues in the field are covered here at least as well, and often better, than anywhere else. Most attention is devoted to topics that in one way or another have to do with the fertility transition. For example, maternal mortality is considered a possible source of motivation for fertility control. Knodel finds that there was a roughly 1% chance of maternal death associated with each confinement, and thus, an approximately 5% cumulative chance of dying as a result of reproduction by the end of the childbearing years; although the mortality risk is low, Knodel speculates that a fear of childbirth may have made some women more receptive to fertility control. He also considers infanticide as an alternative to fertility regulation within marriage but concludes that it was not employed since, when sibship size was controlled, there was little variation in infant mortality by birth order.

Knodel's findings qualify some of the findings of the Princeton European Fertility Project, in which he was a major collaborator. This project relied on rather crude measures of fertility for relatively large population aggregates to describe the onset of the fertility transition. An important finding was the near simultaneity of the timing of the fertility decline across nations and provinces (cantons, counties, etc.). Knodel shows, however, that interpretations of the timing of the fertility decline based on data at the level of the nation or province may be misleading when smaller units are examined. For example, in several of the villages an essentially flat trend in the Princeton project's measure of marital fertility masked counterbalancing trends: steep declines in fertility at the older ages (a sign of control) but rises in fertility at the younger ages. In addition, nearby villages differed substantially in the onset of the timing of the fertility transition. Other findings of the Princeton project are confirmed. For example, Knodel finds that occupational differentials in pretransition fertility and mortality and in the timing of the transitions are relatively small compared with differences among villages or regions.

Sociologists and historians will, I think, find this book recondite and austere. Few concessions are made to readers not acquainted with the techniques of demographic analysis, and only experts will be able to appreciate Knodel's technical virtuosity. Knodel examines the influence of infant mortality on fertility, of birth spacing on infant mortality, of marriage age on marriage fertility. Social structures enter through analyses of differentials by occupation, and culture through village differentials in the extent of breastfeeding and the observance of prohibitions against marriage during Lent.

In general, however, Knodel stays close to the genealogies. The worlds of these villages are portrayed as demographic, not social, systems. It is only a slight exaggeration to say that were it not for the identification of

Book Reviews

the data set as genealogies from German villages and some references to books in German, it would be hard to know what country one was reading about. In many analyses the villages are grouped (e.g., Baden villages, Bavarian villages), but we are told little about Baden or Bavaria. Similarly, the basic temporal distinction is pretransition versus posttransition. Since we learn little about other correlates of this divide, it would seem to matter rather little whether we are talking about 19th-century Germany or 20th-century Thailand.

Both in the introduction and in the conclusions, Knodel makes it clear that his purpose is to demonstrate the use of family reconstitution techniques to explore a far wider range of demographic behavior than was previously possible. A less explicit agenda is to provide "lessons from the past" for those interested in fertility decline in the Third World. Knodel's assumption, probably correct, is that the more the demographic relationships are stripped of contextual details, the easier it is to ship them elsewhere. Knodel has focused on those demographic relationships that are the least likely to be influenced by the specifics of time and place. This means focusing on aspects of demographic behavior that are most closely linked to biology and most distant from local social structures and cultures.

This book can be faulted only for not doing what it did not set out to do. In its own terms—which are widely shared by demographers—it is a major contribution, and it sets a standard that will be difficult to exceed.

Families and Social Networks. Edited by Robert M. Milardo. Newbury Park, Calif.: Sage Publications, 1988. Pp. 237. \$28.00 (cloth); \$14.00 (paper).

Norval D. Glenn
University of Texas at Austin

Interest in social networks is not new among family social scientists. Early in this century it was recognized that one's friends and kin tend to influence the choice of a spouse and the degree of stability of one's marriage, and in the early 1950s C. C. Zimmerman and his associates conducted some rather sophisticated research on the effects of social networks on families. However, the relationship between social networks and families has become a major area of activity in family social science only in the 1980s, as the more sophisticated theories and methods of social network analysis developed in the 1960s and 1970s have been brought to bear on the study of family relations. Although the social networks perspective is a uniquely structural, and thus sociological, approach to family relations, the main impetus for renewed interest in social networks and families has come from psychologists in interdisciplinary departments

American Journal of Sociology

and programs, such as Individual and Family Studies at Pennsylvania State. *Families and Social Networks*, which exemplifies some of the best work that is being done on the topic, includes only one contributor with an appointment in a sociology department, and that person is primarily a psychotherapist.

Family sociologists, who have approached the study of the family primarily from a demographic perspective or from cultural and social psychological perspectives not involving social networks, will, however, benefit from reading this book since family relations cannot be fully understood without taking social networks into account.

The book has nine chapters, the first being an overview of theory and methodology by the editor, Robert Milardo. To the reader with little knowledge of the topic, this chapter will be the most valuable. It should convince anyone that understanding the influence of social networks is essential to understanding family relations, but it may also create skepticism about the adequacy of most measures of the characteristics of social networks. For instance, it reports abundant evidence that most persons are unable to recall with accuracy their interactions and subjective states, even in the recent past, and thus casts doubt on the validity of much of the social network data gathered through conventional survey techniques. It also makes clear that what Milardo calls "psychological networks" (one's significant others) are distinct from, and seem to be quite different in size and composition from, "interactive networks" (persons with whom one interacts regularly). The latter, according to Milardo, can be accurately gauged only through contemporary accounts of daily activity—a procedure that may make excessive demands on the time and attention of the respondent if used for more than a few days (in my judgment).

The remaining eight chapters deal with diverse aspects of the relationship between families and social networks, with family characteristics being sometimes the independent variable and sometimes the dependent variable. Most of the chapters are primarily reports of original research findings, but one (Catherine A. Surra, "The Influence of the Interactive Network on Developing Relationships") is a review and synthesis of previously reported research, and two dwell on previously reported research by their authors. Carlfred B. Broderick's "Healing Members and Relationships in the Intimate Network" is simply a "think piece," but it is my favorite of the eight substantive chapters. Broderick, a psychotherapist, makes the valid point that therapists, by trying to foster assertiveness and self-actualization in their clients, may often tend to weaken ties to social networks that provide invaluable support. The claim that many psychotherapists promote an extreme kind of individualism that tends to undermine social order and community has been made by several authors (including the authors of the influential *Habits of the Heart*), but the point that such individualism may have adverse effects on the well-being of clients has been less frequently made. Coming from a psychotherapist,

Book Reviews

the argument is more convincing than if it had been made by an outside critic.

I do not find a single chapter uninteresting and unimportant. The more interesting chapters, in my opinion, include "Social Networks and the Transition to Motherhood," by Kathryn McCannell; "Changes in Social Networks following Marital Separation and Divorce," by Marilyn Rands; and "Loneliness: A Life-Span, Family Perspective," by Daniel Perlman. The only selection that is flawed to any important degree is also one of the strongest. Catherine Surra, in her generally excellent treatment of social network influences on relationship development, concludes with some generalizations that she presents as though they were fact, but they are no more than theoretically reasonable propositions supported by some evidence. Random experimentation cannot be used to study the effects of social networks on relationships, and thus conclusive evidence of those effects will not be forthcoming. As in most social research, the best we can hope for is high confidence, based on consistent evidence from different, imperfect methods, that certain effects exist.

Overall, this is a very good book—one that illustrates that the best work in family social science is on a par with the best work in any social science specialty. I wish only that sociologists were making a greater contribution to the important work on families and social networks that is exemplified by the contributions to this volume.

The Changing American Family and Public Policy. Edited by Andrew J. Cherlin. Washington, D.C.: Urban Institute Press, 1988. Pp. vii + 261. \$15.95.

Mark R. Rank
Washington University

Currently all persuasions appear to be profamily. The 1988 presidential campaign was filled with profamily rhetoric from both the Republican and Democratic camps. Strengthening the family has become a hot political topic. Increasing concern is voiced over the percentage of children in poverty, the rising number of female-headed families, teenage pregnancies, day care, and so on. Yet when specific policy recommendations for dealing with these and other family issues are discussed, the consensus breaks down.

One starting point for an intelligent policy discussion would appear to be an up-to-date synthesis of the changes that have occurred within the American family over the past several decades, the determinants of such change, and the role of government in affecting family change. *The Changing American Family and Public Policy*, edited by Andrew J. Cherlin, provides such information in a timely and thought-provoking manner.

American Journal of Sociology

This collection examines the changes that have occurred in the lives of family members. Separate chapters are devoted to the trends in the well-being of children (Nicholas Zill and Carolyn Rogers), adolescents (Frank Furstenberg, Jr., and Gretchen Condran), mothers (Cynthia Epstein), and fathers (Furstenberg, Jr.). In addition, there is an overview of the changes occurring in the American family by Andrew Cherlin, and a final chapter by Mary Jo Bane and Paul Jargowsky that examines the links between government policy and family structure.

In most cases, the authors focus on the demographic, social, and economic changes that have occurred to various family members, the reasons behind such changes, and the effect that government policy has and can have on the family. The authors are to be commended for their clear, well-reasoned synopsis of the available social scientific research. While the changes themselves are fairly straightforward to document, the reasons behind such changes (including the role of government policy) are obviously much more difficult to ascertain. Nevertheless, the reader is left with a richer understanding of the potential causes behind changes in the American family.

The chapters by Cherlin and by Bane and Jargowsky are particularly well done. In a fairly short space, Cherlin provides an excellent summary of the major changes that have taken place in the American family. The concluding chapter by Bane and Jargowsky addresses the key question regarding these changes: What has been the effect of government policy on family structure? Their important conclusion is that the role of government policy on the family has been negligible. Rather, "the most important causes of changes in family structure lie outside the realm of policy, perhaps in the profound changes in social attitudes about what constitutes acceptable behavior" (p. 220). Such a conclusion negates the fears of conservatives who are apprehensive about the harmful effects of the government upon the family but also the optimism of liberals who see family policy as a way to solve the troubles within the American family.

Nevertheless, Bane and Jargowsky conclude that government policy can play an important, albeit limited, role in the family's well-being. That is, "government policies can make it easier to form and support families, can reinforce the society's care and concern for children and the families that raise them, and can alleviate some of the ills of disadvantaged families" (p. 250). By doing so, the government will be making a long-term investment in our nation's future.

An important topic not addressed in this volume (raised most notably in the writing of Gilbert Steiner) is of whose needs and concerns (children, mothers, fathers) should take priority when formulating policy. Certainly the needs of these three family members may not always be in sync with one another. How does one, or can one, formulate a policy that avoids this dilemma? Had the authors addressed this difficult but pertinent question, there would be greater coherence among the chapters.

Nevertheless *The Changing American Family and Public Policy* pro-

Book Reviews

vides an excellent overview of the changes, causes, and effects of government policy on the family. It provides information that is vital to both academics and policymakers interested in the changing American family.

Social Science in Government: Uses and Misuses. By Richard P. Nathan. New York: Basic Books, 1988. Pp. xiv + 224. \$18.95.

Martin Bulmer

London School of Economics and Political Science

Daniel Patrick Moynihan, now the most senior American politician with a sociological background, argued 20 years ago that the social sciences had not produced conclusive results in attempting to establish the causation of social phenomena. In conducting research for policymakers, they should concentrate instead, he argued, on studying the *effects* of social interventions, about which it was possible to reach more definite and useful conclusions. This view was expressed at a time when applied social research was in the ascendant. Large-scale field experiments in social policy, growing use of evaluation research, and a growing social science capacity in the General Accounting Office all followed. The Manpower Demonstration Research Corporation (MDRC) in New York City, of whose board Richard Nathan is chairman, was created in 1974 with support from the Ford Foundation and now has over 100 staff. It conducts demonstration studies throughout the United States to test new social programs for the disadvantaged. *Social Science in Government* is about aspects of this recent growth in applied social research.

Nathan, a political scientist at the Woodrow Wilson School at Princeton University since 1979, was on the staff of the Brookings Institution from 1972 to 1979, after service as an official at a high level in the federal government in the 1960s and early 1970s. His book is thus written from an intimate acquaintance with the interconnections between the academic world and government, and a realistic assessment of the promise and performance of the social sciences in the public sphere.

Nathan quotes one of his students, who remarked at a seminar that "liberal advocates of social programs have shot themselves in the foot by emphasizing studies that often show the limits and pitfalls of social programs and only rarely their successes" (p. 18). The tone of the book is positive, seeking to show how such studies can contribute to public policy, but the strength of the conservative critique of applied social research is recognized; indeed the second chapter is entitled "The Rise and Fall of Applied Social Science." He quotes a riddle posed by Aaron Wildavsky: What is the difference between the New Deal and the Great Society? Answer: evaluation research. The optimism with which in the 1960s economists in the federal government embraced evaluation led to some notable field tests of candidate policies—in income maintenance, housing,

American Journal of Sociology

and health—but many of the studies showed small or negative effects of the policy intervention. For example, the Seattle-Denver income-maintenance experiment found an association between participation in the program and marital break-up. This experience could even lead one to argue, cynically, that social scientists should select for testing those policies they dislike most, since the outcome of evaluation in this period was often to undermine their chances of adoption.

The prototypes of demonstration research were the negative income-tax experiments, which are carefully evaluated, with the conclusion that they were not appropriate as a means of testing a universal program that would be highly visible if introduced nationally. They were designed by researchers and did not fit well with the needs of politicians. And the dominance of economists meant that the sociological and psychological understanding gained from the studies was small. Demonstration research, Nathan argues, needs to be designed differently and to focus on the introduction of service-type programs that are more specific and local.

This is exemplified by the supported work demonstrations mounted by the MDRC in New York, which embodied a different approach to helping poor people enter the labor force. Rather than focus upon income, these projects used services such as job counseling, training, remedial education, and child care to support people starting work. The first controlled comparison study showed a considerable effect of such support on the labor-market experience of mothers receiving AFDC, but slight effects for problem youth and previous offenders. This study led MDRC into studies of work/welfare programs at the state level, an important part of the Reagan approach to welfare issues in the early 1980s.

One of these studies, in San Diego, served as a small-scale demonstration of procedures that were then embodied in the California Greater Avenues for INdependence (GAIN) program, a rare combination of liberal and conservative elements in legislation designed to facilitate the entry of AFDC mothers into the labor force. In 1986 MDRC then began an evaluation of the effects of this legislation at the state level, the outcome of which is too near for a report of definite conclusions.

Social Science in Government sets out clearly the differences between a demonstration and an evaluation project. The author's argument is that social scientists should focus more of their attention on the implementation of policies by designing research that can examine the effects of policies once they are under way. One chapter reviews in detail the main methodological features of a demonstration project and pays particular attention to the scope for random assignment in such studies, suggesting that it is greater than is often supposed. The emphasis upon the political context is also welcome. Nathan points out that much social science research for the federal government founders on what he calls the "federalism reef." The sharing of powers with states and counties, particularly in the welfare field, has major implications for policies promulgated initially at the federal level.

Book Reviews

This is an engaging and illuminating book, which will be read with profit by all interested in the relationship between social science and government. Clearly the field is not standing still despite less support at the federal level. Indeed, indirectly the book is a reminder of the pluralism of research support available in the United States. The tone of the book is in places rather bland, and the discussion of MDRC understandably very positive, but these are minor blemishes. Richard Nathan has added to our understanding of the difficult issues in making social science more effective in influencing policy.

The Politics of Social Policy in the United States. Edited by Margaret Weir, Ann Shola Orloff, and Theda Skocpol. Princeton, N.J.: Princeton University Press, 1988. Pp. xiii + 465. \$52.50 (cloth); \$14.50 (paper).

Francis G. Castles
Australian National University

If the Chicago School in economics is on the Right and celebrates the unconstrained market, what appears to be a Chicago School of historical social policy analysis belongs very firmly on the progressive Left. This set of essays, which is perhaps the most comprehensive exposition yet of what this school has to offer, deploys serious scholarly inquiry to derive practical policy recommendations. These recommendations are for a more universalist system of social protection that attacks the historical bifurcation of American social policy into social security and welfare.

The disconnected and fragmented character of American social protection is the central feature of nearly all the contributions to *The Politics of Social Policy in the United States*. But, unlike many of its predecessors, the Chicago School seeks not merely to describe the pathologies that have emerged from the divided character of social policy but also to explain its origins, dynamics, and possible development.

Explaining policy outcomes is a growth industry, but what distinguishes the Chicago School is a coherent paradigm of the nature of social policy development. The editors explicitly label competing paradigms—the industrialization process, societal values, class, and social democratic politics—as at best partial and at worst irrelevant as explanations of American social policy exceptionalism. What they advocate is an “institutional-political process” perspective focusing on state formation, institutional leverage, and policy feedbacks.

What made the United States different is a unique pattern of state formation from its constitutional legacy, a peculiar experience of involvement in war and an early democratic mobilization. The key to continuing diversity is policy feedbacks, which effectively constrain reform efforts into a predetermined and limited range of patterns. In other words, the existing social policy system itself becomes an aspect of state formation and delimits the possibilities for change.

American Journal of Sociology

The pivotal historical conjuncture for analysis is the New Deal period. The essays are divided into three parts. First are those taking a macro-view of the development of the United States's basic policies in the 1930s and early 1940s. Themes here include the question of why America was a social policy laggard (Orloff), why America's wartime experience failed to lead to a consolidation of the New Deal when in the United Kingdom the war was the catalyst for a fully fledged welfare state (Ed Amenta and Theda Skocpol), how pressures emerged for an expansion of private rather than public sector welfare (Beth Stevens), and why the experience of the Depression and New Deal was not transformed into a government commitment to manage the economy in such a way as to produce full employment (Weir).

Part 2 examines programmatic development and innovation within the boundaries established by the New Deal: the development of Old Age Assistance (OAA) and the food stamps program as instances of the leverage of agricultural interests (Kenneth Finegold), how changes in the OAA system between 1935 and 1972 were conditioned by the political leverage of southern interests in a low-wage economy (Jill Quadagno), and an essay by John Myles examining how old-age social security has been transformed into a retirement wage like those of Western Europe.

Part 3 focuses on change since 1960, with a strong emphasis on the changing situation of American blacks. Skocpol, in a fascinating essay, shows how the contemporary policy agenda, particularly that of the Democratic party, is riven by tensions arising from the social policy bifurcation inaugurated by the New Deal, from the failure to meld full employment and social spending into a comprehensive welfare-state structure, and from promises held out to the blacks to include them in the social policy collectivity from which they were excluded by the New Deal. Other essays discuss the liberals' abandonment of structural explanations and solutions for racial inequities in social provision (Gary Orfield), the different fates of Medicaid and subsidized housing as targets of the Reagan budget axe (Helene Slessarev), the feminization of poverty (Mary Jo Bane), and the way in which unemployment—particularly of blacks—has impinged on family structure and poverty (Kathryn M. Neckerman, Robert Aponte, and William Julius Wilson).

Because the essays present a cogent interpretation and explanation of where American social policy is, it will be prominent in academic debate and become a standard graduate text. That is far from saying, however, that the volume is without flaws.

Two fundamental objections should be registered, even though the institutional-political perspective is a potentially valuable corrective to ahistorical, generalizing theories about the origins of the welfare state. The first is a point implicitly noted by John Myles: debates about the origins of the New Deal are more about proximate forces (agents and intentions) than about fundamental sources of change. Like most advocates of novel paradigms, the Chicago School tries to explain too much in

Book Reviews

terms of their model, leading to an outcome that is simultaneously over- and underdetermined because social policy is explained by one factor rather than through a causal chain constituted by the interaction of many.

The second objection is to a tendency to cross-national comparison, which is often shallow to the point of being misleading. The volume is not comparative in focus, but its central problematic—why American social policy is atypical—most certainly is. Moreover, the conceptualization of the problematic is crucial to the elaboration of the explanatory paradigm, for if American social policy were not atypical there would be less need to adduce a unique institutional-political process to explain it. But the exceptionalism of American social policy cannot be accepted by fiat, as Myles alone among the contributors here shows. In some areas clearly America is startlingly different. The absence of comprehensive public health care is the most flagrant example, which makes it most peculiar that no essay was explicitly devoted to this topic. On the other hand, in the realm of social transfers the United States is far from being as exceptional as often portrayed. In a recent compilation of OECD data on 20 countries (*The Government Household Transfer Data Base, 1960–1984*), 10 averaged more than 8% of GDP on transfers in the period 1960–84, and 10 less than 8%. Of these latter, the United States ranked fourth, with 7.2%, identical to Canada and a bigger spender than Switzerland or Australia. To explain why the United States in this respect is a typical lowish spender might require a reorientation from the exceptionalism of the American institutional context to what it has in common with some other countries.

Remaking the Welfare State: Retrenchment and Social Policy in America and Europe. Edited by Michael K. Brown. Philadelphia: Temple University Press, 1988. Pp. viii + 312. \$34.95.

Stanley DeViney
University of Kansas

This collection of papers is derived from a conference, The Unraveling of the Welfare State, held in 1985. The stated goal of *Remaking the Welfare State* is a comparative analysis of welfare-state trends in the past decade, but the underlying theme is the nearly eternal question in this literature, Is the United States a unique case? Of the fourteen chapters, eight deal with the United States alone and only two can be said to be fully comparative. No nation, aside from the United States, is the subject of more than a single chapter. Nor are all European nations covered. Noticeably missing are the Scandinavian nations, once considered the models of the welfare state.

The one-sided use of material drawn from the American experience and the limited number of truly comparative essays do not mean that the

volume is devoid of unifying themes. Several weave their way through the volume. These themes are nicely presented in the introductory chapter by the editor, Michael K. Brown. The first is that cuts, or at least declining growth, in welfare-state programs have not been uniform. They have been made mostly in family allowances and unemployment benefits, while pensions have remained largely untouched. This has resulted in a two-tier welfare system, with programs intended for the middle class left in place and programs for the poor reduced. A second theme is a shift in the social base of support for welfare-state programs. The resistance to cuts in programs has come not from working-class groups but from professional and institutional interests often centered in state organizations that provide services and benefits. The third theme is near universal acceptance of anti-welfare-state ideology and the lack of an intellectual statement in support of the welfare state.

A major element of the current anti-welfare-state ideology is the perceived negative consequences of transfer payments for economic growth. In chapter 2, Roger Friedland and Jimy M. Sanders provide a strong empirical attack on this proposition. In an analysis of industrially advanced nations, Friedland and Sanders report that transfers to households contribute to economic growth, while transfers to firms and businesses are likely to retard growth. Hence a central principle of the conservative attack on the welfare state is found to be questionable. This still leaves open the question of why the proposition of a linkage between the welfare state and slowed economic growth has been nearly universally and uncritically accepted in both the United States and Europe.

The second theme, the new social basis of support for welfare-state programs, can be found in the chapters by Frances Fox Piven and Richard A. Cloward, Enzo Mingione, and Steven Rathgeb Smith and Deborah A. Stone. Piven and Cloward argue that the growth of social programs in the 1960s and 1970s has brought with it a growth in employees in social welfare agencies. It is these civil servants who form the infrastructure of support and defense of programs. Smith and Stone add to this theme with their statement of the unintended consequences of the privatization of social welfare. Private agencies, intended to reduce government costs, are forced into a position of advocating sustained and increased programs as a means of ensuring their resources and programs. Mingione reports a similar pattern in Italy, a nation that has not experienced the retrenchment of other Western nations. The Italian system has developed as "a conveyor belt between fragmented interest groups . . . and welfare state strategies." This system has produced a form of clientelism that is difficult to reduce.

The dominant theme that cuts through most of the chapters is that the present welfare state is segmented into universal programs that include middle-class beneficiaries and means-tested programs intended for the poor. Nearly all the authors report that cuts have been in the latter, while the former either have remained largely unchanged or have grown. The

Book Reviews

collective "surprise" of the authors at this finding is largely due to their acceptance of a class-based model of the welfare state. If the authors would consider another basis for welfare-state growth, particularly the work of Fred Pampel and John B. Williamson, they could have appraised the role of other stratification factors, especially the political power of age groups that cut across class lines, that would support universal programs. Pensions, with strong ties to age-based politics, have always dominated welfare-state programs, with means-tested programs placing a distant second. Hence the decline of working-class organizations and parties would not be thought a major threat to pensions.

No edited volume can be expected to have a consistent quality of papers. I have serious questions only about the inclusion of William Darity and Samuel Myers's review of the consequences of budget cuts for blacks, which could be perceived as bordering on the paranoid. The authors suggest that advocates of birth control and abortion liberation represent "whispers of genocide" (p. 286). But it is these programs that were cut by the Reagan administration, and as a consequence the choices of black females were also cut. This chapter, reprinted from the *Urban League Review*, detracts from a good collection of case studies of welfare-state retrenchment.

Class Struggle and the New Deal: Industrial Labor, Industrial Capital, and the State. By Rhonda F. Levine. Lawrence: University Press of Kansas, 1988. Pp. xi + 233. \$25.00.

Edwin Amenta
New York University

As the 1930s ended, Americans were oblivious to the prospects of war. After all, America was protected by oceans, but unemployment was still in double digits. Scholars, politicians, and the public debated the causes of the Depression, whether prosperity would return, and whether the New Deal was having any effect.

In *Class Struggle and the New Deal*, Rhonda F. Levine addresses all this and more. The monograph explores the reasons for the economic crisis, the ways out of it, the determinants of New Deal policies and state policy-making in general. It purports to advance one theoretical perspective while discrediting another through a narrative of policy-making highlights and debates. It surveys a wealth of historiography, government documents, and archives. The book, however, collapses under the weight of its ambitions, none of which are realized.

Levine's thesis is that the New Deal produced political consequences promoting a resurgence of economic growth, or, as she puts it, allowing monopoly capital to accumulate. She claims that the 1930s American political economy suffered from three problems: too high a rate of exploi-

tation, unregulated competition, and "a political structure that was not conducive to the imperatives of the accumulation process" (p. 4). By 1939 the New Deal had solved these problems by effecting "three important political consequences: (1) the unchallenged political hegemony of monopoly capital; (2) the diffusion of labor militancy into general ideological consensus for capitalist rule; and (3) the incorporation of organized industrial labor into the national process of political bargaining" (p. 4). Drawing on Nicos Poulantzas, Levine approaches New Deal policies with a "class-centered" model: state policies reflect and respond to conflicts between monopoly capital, competitive capital, and labor. The book reviews economic developments from 1890 through the Depression and recounts several episodes of policy-making: the (1933–35) National Recovery Administration (NRA), the debates over it and the Temporary National Economic Commission of 1938, the struggle for union organization and protection (notably in the 1935 National Labor Relations Act), and Roosevelt's bid to reorganize the executive.

By claiming that certain conditions were necessary for monopoly capital to accumulate, Levine implicitly builds a theory of the causes of the Depression and the American economic resurgence. However, she fails to indicate how the economic problems she emphasizes were more crucial than others. Unregulated economic competition may have inhibited capital accumulation, as she claims, but how had this changed by 1939? Some of the supposed political requirements of economic growth seem unnecessary. Monopoly capital in Japan flourishes without incorporating industrial labor in political bargaining.

Her indifference to bolstering her functionalist contentions stands out on the issue of timing. Levine asserts that the political requirements for capital accumulation were established by 1939. Opposing standard accounts of the foundations of the postwar American economic boom, she implies that if the war had never happened, the American Depression would have ended anyway. Some authors point to the economic stimulation of huge government purchases for war. Others stress international causes: the American-dominated international monetary regime, free-trading agreements, the devastation of America's economic competitors. Would there have been an economic resurgence without these wartime changes? Not only does Levine supply no counterfactual scenario—standard arguments go unmentioned—but nowhere does she discuss Keynesianism or the famous Baran and Sweezy monograph, whose title figures prominently in Levine's book. What is more, the unionization of industrial labor and its decline in militancy, supposedly key political requirements, were still in doubt in 1939. The share of the unionized labor force more than doubled between 1939 and 1946; collective bargaining was not institutionalized until wartime, when strikes surged.

The policy-making episodes are often summarized well but are unsuited to appraising theories. Much space is devoted to the failure of the NRA. Unfortunately, Levine's model predicts the same outcome as

Book Reviews

Theda Skocpol and Kenneth Finegold's state-centered perspective, which Levine hopes to replace. They argue that the NRA failed because of a lack of state capacities in this policy area. Levine counters that the interests of monopoly and competitive capital were at odds and that these "contradictions" were reflected in an unsuccessful state policy. This is, if accurate, a supplement rather than the refutation one expects from the polemics. Perhaps reorganizing industries was more difficult than constructing a successful agricultural policy, Skocpol and Finegold's comparative case. Yet Levine misses a similar analytical opportunity by failing to consider industries where the codes of competition were somewhat successful, as in bituminous coal, oil, and transportation. Because the NRA was dismantled in 1935, Levine is no closer to depicting the lasting reforms of the New Deal or explaining their importance, and the book is half-finished.

The final episode, on state reorganization, underlines the book's analytical difficulties. Levine rejects the conventional wisdom that Roosevelt's bid to reform the executive branch was an out-and-out failure. From this promising start, however, she makes unsupported assertions connecting these reforms with class contradictions and capital accumulation. She asserts that a stronger executive serves the interests of monopoly capital. A strengthened executive can more easily dominate Congress, where competitive capital supposedly rules. Therefore, the reorganization plans promoted the "hegemony of monopoly capital." From here she claims that although the political voice of monopoly capital was silent—monopoly capital being unable to perceive its true interests—this silence somehow indicated that reorganization was really a matter of intracapitalist conflict. This was true despite the fact that members of the executive branch—remember, the unwitting ally of monopoly capital—fought reorganization. Such argumentation will convince only the already converted.

American Neighborhoods and Residential Differentiation. By Michael J. White. New York: Russell Sage Foundation, 1988. Pp. xx + 327. \$37.50.

Avery M. Guest
University of Washington

As Michael J. White, the author of *American Neighborhoods and Residential Differentiation*, notes, the research literature on residential patterns in U.S. cities is extensive. As is true in many areas of social research, most of this literature developed after World War II, with the "golden" pioneering days of the 1950s and 1960s.

To speak quite frankly, the ready availability of census data may help explain the appeal of analyzing residential patterns in cities. Extensive tabulations of population characteristics for numerous census tracts and

residential blocks have been a heaven on earth for those of us who like wallowing in cheaply purchased numbers. Unfortunately, even with our numerous studies, many of us have failed to take seriously Robert E. Park's idea that the study of spatial distances is important as an indicator of social distances. The social causes and consequences of segregation have simply received little attention relative to its description. There are, of course, some striking exceptions such as the "classic" work of the Duncans, the Taeubers, Stanley Lieberson, Leo Schnore, and the "contemporary" work of John Logan, Douglas Massey, and Harvey Molotch.

In my opinion, advances in our understanding of the causes of segregation will have to relate the spatial distribution of the housing stock (such as the age, size, and value of housing) directly to the distribution of individuals by familial, socioeconomic, and ethnic characteristics. In addition, it will be necessary to move beyond the available census data by considering such factors as the political and economic structure of communities, the symbolic meanings attached to them, the locations of workplaces, and the roles of institutional actors. The agenda is obviously large, but we already know a lot about the basic patterns of segregation.

White's monograph tackles residential segregation primarily from a descriptive rather than a causal framework. Almost certainly, his study will be considered a basic source for learning who lives where but may be less satisfactory for understanding why they live there. It concentrates on describing differences in levels of segregation across various population dimensions such as socioeconomic, familial, and ethnic statuses. In addition, it presents some interpretation of differences across metropolitan areas. In this descriptive orientation, it pursues an explicit goal of the associated monograph series on the 1980 U.S. Census.

Given the vast array of 1980 census-tract data for over 300 metropolitan areas, White made a wise strategic decision to concentrate on 21 purposively selected places, representing a variety of regions, size groups, and growth rates. For many of these areas, historical data were available with reasonably consistent tract boundaries; the areas also correspond frequently to cities used in previous ecological research.

After describing his data and sample, White initially concentrates on describing basic differences in residential differentiation for various population characteristics. He then analyzes the distribution of characteristics by distance from the center of the metropolis, sectors or wedge-shaped slices of the metropolis, and nuclei or nodal points. Finally, for selected metropolitan areas, White investigates some of the temporal dynamics of change between 1970 and 1980.

While the monograph is chock-full of findings, readers who are not specialists in the area will face difficulties in figuring out what is new. White makes little reference to the extensive literature on related topics. Except for a few references to general and often dated theoretical pieces, White often writes up his results as though he had just invented the empirical study of residential patterns in cities. In addition, theory is

Book Reviews

often used very casually to explain differences across metropolitan areas and population characteristics.

Perhaps the major substantive conclusion of the study is that the metropolis is no longer a monocentered entity in which population characteristics can be arrayed clearly in terms of distance from the traditional central business district. Related to this is the important finding that neighborhood life cycles are varied in their natures and not closely related to the age of the community or its spatial location in relation to the city center. These findings underline the need to seek causal interpretations of residential patterns other than those suggested by a mildly modified version of the Burgess "growth hypothesis." Unfortunately, White does not come up with any new ideas about the causes of metropolitan development. But, then, I have not either.

There are also a few interesting surprises in the empirical findings. For instance, while distance from the metropolitan center fails to explain much of the spatial distribution of higher-status groups, the monograph shows that they are also only weakly located in the sectors or wedges that economist Homer Hoyt emphasized in his formulations about urban development.

In summary, White's study must be considered a solid report on where people live in the metropolis. The questions of why they live there and how residential distributions are changing need much more attention. One can charitably say that there is plenty of work left for White, myself, and many others. And the data are cheap, too.

Borderland: Origins of the American Suburb, 1820-1939. By John R. Stilgoe. New Haven, Conn.: Yale University Press, 1989. Pp. xiii + 353. \$35.00.

Adam Bickford
University of Chicago

Borderland is a social and architectural history in the same vein as Mark Girouard's *Cities and People* (Yale University Press, 1985), Spiro Kostof's *America by Design* (Oxford University Press, 1987), Gwendolyn Wright's *Building the Dream* (MIT Press, 1981), and Robert Fishman's *Bourgeois Utopias* (Basic Books, 1987). Each of these books examines a particular aspect of the built environment through the intellectual and ideological texts that surrounded and justified its development. The type of environment John R. Stilgoe addresses is upper-class exurban and suburban settlement between 1820 and 1939. In surveying the writings of landscape architects, urban reformers, novelists, diarists, and "gentlemen farmers" of the period, Stilgoe argues that the suburban towns and villages settled before the end of World War II were of a different character from those settled after, his point being that lumping the two types of

settlements together under the single label "suburban" obscures the variety of life-styles in the places between the cities and the rural hinterlands. Pre- and post-World War II suburban settlements differ significantly in the sizes and social classes of their populations, in the design and construction of their homes, and in the role of the suburb in the urban and regional economy. To distinguish between the two, he applies the term "borderlands" to describe the earlier type of suburban settlement, leaving suburban to describe the postwar landscape.

Stilgoe's classification of suburban towns and settlements as borderland towns relies entirely on whether a town was settled and developed before the end of World War II and, one assumes, before the passage of the FHA and GI Bill home-loan programs in 1949. This is such a simple distinction that it is not surprising that Stilgoe is not able to sustain any argument about the distinctiveness of borderland settlements. He uses the term "borderlands" indiscriminately, applying it to nearly every type of pre-World War II nonurban residential settlement, from the streetcar suburbs of Philadelphia to the farms of retired capitalists in upstate New York to planned residential communities such as Forest Hills Gardens, also in New York (which was originally developed by the Russell Sage Foundation). Nowhere does he compare borderland towns with later suburban towns, nor does he offer any extended description of how the borderlands differ from either rural areas or cities.

Another problem that Stilgoe encounters is with his use of sources. The book is organized into 24 short chapters, and each chapter contains between one and nine short anecdotes about the chapter's subject culled from Stilgoe's collection of novels, diaries, and articles. A chapter may move from a discussion of journal articles on horticulture published in the 1840s, to a discussion of a novel written in the 1920s about city people learning how to garden, to a discussion of a diary written by a gentleman farmer at the turn of the century that describes how to transplant and grow Chinese trees. This style of writing may be faithful to the disconnected nature of the available materials, but it is frustrating to read.

Further, Stilgoe does not discuss anything *but* his sources and ignores the effects of the development of American society on the creation of the ideas and rhetoric about early suburban growth. The period Stilgoe is writing about is the initial period of American industrialization, yet he makes only oblique references to the rise of the middle class, to the dynamics of land speculation and economic growth, and to seemingly important events such as World War I and the Great Depression. In addition to this general disregard of social and historical context, Stilgoe ignores all other scholarship on American urban history and urban sociology and seldom cites any works outside his collection of period sources.

Despite these problems with definition, argument, and the use of sources, Stilgoe has assembled a fascinating array of documents from the early history of the American residential landscape. Of particular interest is part 2 of the book, "A New Sort of Space: A Proper Country Home,"

Book Reviews

which is a discussion of the work of Andrew Jackson Downing in the development of a specific romantic ideology of the countryside. This ideology provided much of the justification for the migration of the urban upper classes to rural towns. Also of interest is part 5, "The Planned Residential Community," which is a comparison of the development of Forest Hills Gardens and Shaker Heights, Ohio. In addition, the minutiae of the different sources will give urban history browsers a lot of pleasure.

America Becomes Urban: The Development of U.S. Cities and Towns, 1780-1980. By Eric H. Monkkonen. Berkeley and Los Angeles: University of California Press, 1988. Pp. xvi + 332. \$25.00.

Harvey Molotch
University of California, Santa Barbara

Urban sociologists tend not to credit local government as a force shaping the city, favoring instead ecological imperatives, economic exigencies, or social movements. Historian Eric Monkkonen thus runs against the sociological tides when he argues in *America Becomes Urban* that the key factor affecting U.S. urban development has been the changing nature of local government institutions.

The main shift, according to Monkkonen, has been from a regulatory city to a service one. The regulatory city determined who could practice a trade and set price limits in the local marketplace but took no responsibility for providing urban services. If a child was lost or a crime committed, neighbors were mobilized or voluntary brigades called on. Beginning in the mid-19th century, cities established professional police and fire departments and public school and library systems. Liberating their own markets from government control, cities also became aggressively entrepreneurial, merchandising themselves to the railroads, canal builders, and investors of all sorts. Public funds were used to subsidize future growth, and the booster spirit reigned supreme.

Monkkonen likes these changes and reports being surprised at his discovery of so many positive tendencies in the history of cities. His happy story is due, I think, to his identifying the two master trends (growth and service) as being one and the same. For him, the emerging city invests more to grow more, and this provides the resources to serve populations better. This benign view of development parallels the argument of political scientist Paul Peterson (whose work Monkkonen seems not to know) that urban growth is a public good serving all classes and races alike.

Urban sociologists (and historians) have been learning, of course, that this picture "ain't necessarily so." The historian Olivier Zunz (whose work is otherwise appreciatively cited by Monkkonen) reports, for example, that Detroit's leaders invested in utilities for money to build new

American Journal of Sociology

residences for the affluent, while already-built poor neighborhoods suffered. Joe Feagin's analysis of contemporary Houston indicates how that city's growth boom only exacerbated the problems of the ghetto and indeed now leaves the whole metropolis in a morass of problems. Environmental impact reports attest to the likelihood that growth projects often hurt the poor and sometimes "do in" the whole city.

In the first sentence of his first chapter, Monkkonen asks why "the most fundamental aspects of American urbanization" (among which his first item is "population growth") have "been analyzed so little" (p. 8). But by hanging his own analysis on the expansion of urban services, he misses the opportunity to go very far toward rectifying the problem. He ignores two topics that need to be central to a history of "the development of U.S. cities and towns": how the property business operated over time and how growth benefits were distributed across race and class.

These omissions seem especially odd given the author's frequent mention of "speculators" (p. 205) and the real estate industry as being crucial to "the very nature of twentieth-century infrastructural city services" (p. 93). He mentions that George Washington led a procession to "drum up land sales" in the District of Columbia, but the only motivation he gives is the desire "to encourage residential settlement as a more seemly alternative" to boardinghouse use (p. 63). There are other tidbits that go undeveloped: prisons, for example, were located "to pay political debts" (p. 105), but no analysis appears on how locational decisions intersected with politics. As an informant in one of my own studies once indicated, "a sewer line is a route of power"—a notion very foreign to the spirit of this book.

Monkkonen's lack of attention to distributional effects perhaps stems from his belief that since "real property ownership was possible for even the working poor" (p. 127), everyone had a vested interest in growth. But the data he cites (p. 282 n. 39; pp. 198–99) indicate only a 26% home ownership rate in 1900 and still less than 50% in 1930. Even today a significant plurality of Americans have no stake in the property appreciation putatively associated with growth.

Although Monkkonen convincingly argues that many kinds of urban services have improved over the past 100 years, I do not find this fact a very useful analytic handle for organizing a history of urban America or for pointing the field in new directions. The service improvements he cites were unique neither to cities nor to America; in societies with less government decentralization they came about anyway and transformed rural as well as city life (indeed, in Western Europe most urban services are superior to those in the United States, even though provided heavily by national rather than local government).

Although I am disappointed with its primary thrust, *America Becomes Urban* contains many illuminating insights. Monkkonen emphasizes, for example, the extraordinary geographic mobility that has always been present in U.S. cities (even small towns), countering the romantic view of a residentially stable prewar America. On the basis of high rates of vio-

Book Reviews

lence in low-density 18th-century cities, he wisely argues against the physical determinism that attributes present-day social ills to high-rise residence.

The weakness of the book is a lack of a compelling argument to tie the insights together. Monkkonen's primary error is his anthropomorphizing "the city" into an internally undifferentiated actor on the historic stage. Just one page after his complaint about other authors' doing this, he says that St. Louis was "lamenting its failure to become the population center it had aspired to be" (p. 45). Who in St. Louis was doing the lamenting/aspiring, and specifically what did they do about it, how did they do it, and with what social effects? Cities were shaped, as Monkkonen states, by the process of speculating and deal making. Why then, to repeat the author's initial question, is there so little attention in this book to "the most fundamental aspects"?

Metropolis: From the Division of Labor to Urban Form. By Allen J. Scott. Berkeley and Los Angeles: University of California Press. Pp. xi + 260. \$30.00.

M. Gottdiener
University of California, Riverside

Fifteen years ago a geographer, David Harvey, published a book that helped direct the energies of concerned urbanists to the link between the production of space and the larger social system of political economy (*Social Justice and the City* [Baltimore: Johns Hopkins University Press, 1973]). Subsequent work in urban geography challenged successfully the worn-out paradigm of that field. If Harvey's early book could be considered the beginning of this effort for English-speaking geographers, Allen Scott's recent volume, *Metropolis*, can be considered the end or, at least, one possible end. Scott so distorts the original sensibilities of the new urban geography and deals with its themes so retrogressively that it is difficult to believe that, at one time, this line of reasoning held such promise. Fortunately, other urban geographers are continuing the progressive tradition.

The social justice problematic of Marxist urbanists was not a mere reformist impulse that could then be brushed aside easily by counterarguments based more on laissez-faire political values. On the contrary, the focus on inequity and uneven development, which served as the hallmark of the Marxist approach, followed from a precise political economic formulation of the city-building process. Relying heavily on the devalorization theories of the French School, principally those of Paul Boccardo, the argument asserted that state intervention performed a deep-level function by avoiding crisis through the relative devalorization of capital.

Scott dissented from this Marxist or "neo-Marxist" approach in his early work. As a neo-Ricardian he could not accept the cevalorization

American Journal of Sociology

theory because it assumed certain properties of the law of value that went against Ricardian premises regarding the determination of prices or rents. For Scott both uneven development and the social inequities of the urban system were produced directly by the system of urban rents and capital flows involved in the process of production.

I have assessed Scott's earlier position elsewhere. Briefly put, while he was quite correct to question devalorization theory and hence the theoretical basis for both collective consumption theory and the theory of uneven development, he failed to derive the production of space from a neo-Ricardian theory of rents. Were it not for its trenchant critique of Marxian political economy, this early effort would have been dismissed as blatant Ricardian essentialism. In that work, Scott failed to explain the production of urban form and fell back on outdated concentric zone models of metropolitan growth.

The current book can be viewed as an attempt by Scott to go after the same demons by privileging the same place in capital accumulation but by using a different disguise. In this case Scott once again seeks to show how both the social and political problematic of urbanism and the production of space itself can be derived solely from the conditions of production. Unbelievable as it seems, Scott privileges the division of labor in the production of space in his renewed assault on the new urban analysis and proposes a preposterous essentialism that claims to explain urban form. It is one thing to take the epistemological position that political economy should focus solely on valorization in manufacturing, since this is the industrial base of the economy, but it is quite another to assert that from this place we can derive both the logic of capital accumulation and the production of space itself.

To be sure, there are some benefits to urban analysis in looking at the dynamics of the firm in this way. Principally, and this is the core of Scott's contribution, the spatial arrangements of firms result, to an extent, from the way production units are horizontally and vertically integrated. For example, vertical disintegration *can* lead to manufacturing deconcentration, subcontracting, and hinterland dispersal, while horizontal integration *can* produce the more familiar urban agglomerations that capture externalities in space. Clearly these processes have something to do with the production of space, but Scott claims they have everything to do with it and that the division of labor and its consequences for vertical and horizontal integration simply determine all the production of space.

Scott uses the questionable discursive method of selecting examples that support his view while ignoring other possibilities that may not. Hence he gives the false impression that he is explaining the production of space. This is most evident in his reductionist view of Orange County, which can be understood only by reference to both the local and national states and through the structuration of capital circulation according to both its valorization and realization circuits.

Aside from some interesting insights into industrial geography, this work does not add much that is of value to the critical tradition launched

Book Reviews

in urban studies, and its retrograde argument obscures the important roles played by other equally important factors in the production of space.

Rainbow's End: Irish-Americans and the Dilemmas of Urban Machine Politics, 1840-1985. By Steven P. Erie. Berkeley and Los Angeles: University of California Press, 1988. Pp. xiv + 345. \$27.50.

Kenneth Finegold
Rutgers University

Most books on the machine in American city politics have been angry, amused, or sociological. Muckrakers from Lincoln Steffens to Mike Royko have exposed the venality of machine politicians. Other observers have reveled in the colorful language and exploits of such bosses as George Washington Plunkitt of New York and Richard J. Daley of Chicago. Academic studies of the machine have relied most heavily on sociological explanations. Political scientists such as Daniel Patrick Moynihan, historians such as Richard Hofstadter, and sociologists such as Robert K. Merton have understood the machine as an institutional expression of Irish-Americans' values that performed latent functions for them and for other immigrants.

Rainbow's End, Steven P. Erie's book on machine politics in eight heavily Irish cities (Albany, Boston, Chicago, Jersey City, New York, Philadelphia, Pittsburgh, and San Francisco), combines the first two traditions while breaking with the third. Erie rejects portrayals of the Democratic machines as redistributive institutions or as positive models for contemporary minority politics; he argues convincingly that machine politics did not facilitate economic mobility for the Irish and did not incorporate other ethnic groups. Erie's love of such nicknames as "the Purple Shamrock" (Boston's James Michael Curley) and "Fartin' Martin" (Chicago's Martin Kennelly) enlivens his clear writing, though he adds nothing by repeatedly calling New York "Gotham" and Pittsburgh "the Steel City."

Erie, a political scientist, offers an original version of exchange theory as a substitute for sociological interpretations of the machine. He defines the political economy of the machine by resource scarcity. Material incentives, such as patronage jobs and municipal services, are most effective in inducing citizens to support the machine as voters or activists. But the supply of these rewards is limited. Machine formation and maintenance consequently require that leaders either reduce public demand for political resources or expand their supply. Machines reduced demand by restricting the urban electorate and by offering non-Irish supporters symbolic recognition instead of substantive resources. To expand resource supply, machines forged intergovernmental alliances with state and national officials.

Erie shows that the ability of machines "to fashion a favorable ex-

change ratio between claimants and resources" (p. 14) varied over time and among cities. He divides urban political history into four periods: 1840–96, 1896–1928, 1928–50, and 1950–85. In each era, the cities' ethnic compositions and intergovernmental contexts interacted to create machines with "a distinct electoral coalition and set of policies" (p. 19). Few studies of machine politics have been comparative; most of what we know in any detail we know from case studies of individual cities. Why, Erie asks, did some cities produce machines early, some late, and some not at all? He explains intercity differences by examining relations between would-be machine leaders and state and national officials. Simply by pointing out that Boston, the most Irish of America's big cities, never had a central machine, Erie demonstrates that machine politics did not grow automatically from Irish ways. The inability of Boston Democrats to build a machine is traced to a succession of hostile governors and to Franklin Roosevelt's decision to deny Mayor Curley the federal patronage that could have consolidated Curley's power.

Erie's interpretation of urban politics as the exchange of electoral support for material resources is least compelling when applied to the machine's opponents. For Erie, recent black insurgencies and the reform coalitions of the Progressive Era have been as materially based as the machines they sought to overthrow. Erie argues that Reagan's cutbacks sparked the black revolt against the machine and have limited the resources needed to institutionalize black control. But black politics ultimately arose from the nonmaterial incentives of the churches and the civil rights movement. Pride, not patronage, explains why paintings of Harold Washington were sold on the streets of Chicago.

Erie's discussions of reform minimize the potency of this earlier challenge to machine rule. The extent to which Tammany lost control of New York between 1901 and 1917 is consistently understated, and San Francisco disappears from the historical narrative after 1913, when the city elected the first of the reform administrations that have governed it to this day. Erie asks, and suggests answers to, a perennial question, Why did the machine fail? But this question is inseparable from another one, Why did reform succeed? And this question can be answered only by examining the transformations of reform ideology that, in some cities, allowed reformers to construct coalitions that united elite and working-class sources of opposition to the machine.

New approaches to politics and society have often been worked out in efforts to understand the city. Theoretically, historically and comparatively *Rainbow's End* is a significant and provocative contribution to this double process. Future books will surely depend on it as they draw on political economy and urban sociology to explain the machine and its opponents.

Book Reviews

Searching for Rural Development. By Merilee S. Grindle. Ithaca, N.Y.: Cornell University Press, 1988. Pp. xii + 196. \$24.95.

Douglas S. Massey
University of Chicago

In *Searching for Rural Development*, Merilee Grindle has written an extended critique of theories that stress the primacy of agriculture in promoting national development and of the idea that there is one universally applicable strategy for economic advancement in developing nations. Theorists such as Bruce F. Johnston and William C. Clark (*Redesigning Rural Development: A Strategic Perspective* [Baltimore: Johns Hopkins University Press, 1982]) have argued that national development is best achieved through a rural based strategy that increases agricultural productivity among small landholders by applying appropriate technologies, increasing access to credit, improving technical assistance, building market facilities, and creating a rural infrastructure. These actions allow small farmers to acquire more income, which stimulates demand, generates employment, strengthens links to urban markets, and ultimately leads to dynamic national development.

According to Grindle, strategies designed to enhance smallholder agricultural productivity may have been effective in promoting national development in countries such as Taiwan and Japan, but they ignore the harsh ecological and political realities of most contemporary Third World nations. She develops this critique using Mexico as a case study. In Mexico, as elsewhere, the agricultural resource base is poor and costly to improve, government pricing policies are oriented to the needs of urban consumers rather than rural farmers, land is unevenly distributed, landlessness is increasingly widespread, and agricultural investment funds go disproportionately to large landholders. The book is less an original analysis of new data than an extended literature review supplemented by limited fieldwork in four rural communities that have been extensively studied by other researchers.

Drawing on a voluminous research literature, the author argues persuasively that programs designed to promote agricultural productivity on small farms are unlikely to promote national development in Mexico. She points out that rural Mexico is no longer an agrarian economy. Recent studies universally demonstrate that rural Mexican households are heavily involved in temporary labor migration, both to Mexican cities and the United States, and that this migration has had a marked impact on agricultural production, rural consumption, local investment, and regional development. Borrowing from recent theoretical work by Oded Stark and others, she goes on to argue that this involvement is a rational response by households to an unstable, risky, and inhospitable economic environment. The issue, therefore, is less one of promoting rural agrarian development to dampen migration than of channeling migration and its remittances to promote local employment, both in and out of agriculture.

American Journal of Sociology

Following her consideration of migration and the household economy, she critically evaluates the various policy options for generating employment in rural Mexico. These include enhancing agricultural productivity, altering the structure of Mexico's *ejido* system, creating a rural infrastructure, promoting rural industrialization, expanding agroprocessing, carrying out industrial decentralization, extending the Maquiladora program, and, finally, linking migration to economic growth and development. She then reviews the history of migration and development in her four sample communities and concludes that there is no simple strategy for promoting development across all rural communities. Rather, in a diverse country such as Mexico, development programs must be tailored to local variations in geography, climate, politics, culture, and market potential.

In the final chapters, Grindle details the formidable obstacles to change that lie within the Mexican political economy, pointing out the significant "slippage" (her word) between development theory and practice, especially between policies formulated by leaders at the top of the political hierarchy and their implementation by government bureaucrats and politicians at the bottom. The Mexican state is an extremely stable and very powerful institution, with entrenched interests that tend to bend new policy initiatives to benefit those in the political system, thereby ensuring its perpetuation. Not only is the range of policy options constrained by what interest groups in the ruling coalition find acceptable, but, once put forth, government policies inevitably serve the exigencies of politics first and the needs of rural development second. The Mexican government is itself constrained, in turn, by the strictures of the international economy and by the necessity of reaching an accommodation with the United States and manifold interests.

Throughout the book, Grindle displays an impressive knowledge of the structure, organization, and operation of the Mexican political economy, of the Catholic research literature on Mexico, and of local social and economic conditions. Although *Searching for Rural Development* may not be an original analysis of new ideas or data, it provides an extremely useful summary of current knowledge about conditions in rural Mexico. It is a comprehensive and informed criticism of grand development theories put forth by scholars as blueprints for progress in Third World countries, most of which they have never visited.

Land Reform and Social Change in Iran. By Afsaneh Najmabadi. Salt Lake City: University of Utah Press, 1987. Pp. 246. \$25.00.

Said Amir Arjomand
State University of New York at Stony Brook

The land reform was the first, and by far the most radical, component of the modernization program the shah embarked on in the decades preced-

ing the Iranian revolution. Its consequences were far-reaching. From 1962 to 1972, "Two thirds of the Iranian peasants became beneficiaries in one way or another of about half the total available agricultural land" (p. 3). The land reform resulted not only in considerable land distribution but also in fundamental institutional change. The old agrarian property relations that maintained sharecropping, use rights not tied to specific plots of land (*nasaq*), ownership of entire villages, and absentee landlordism were replaced by tenancy and private ownership of specific plots of land. Further results were the freeing of labor from land on a large scale and the commercialization of agriculture.

The strength of Afsaneh Najmabadi's *Land Reform and Social Change in Iran* is not theoretical but empirical. Although theories of economic development with emphasis on agricultural development and land reform in the postwar period are surveyed at some length (chap. 2), their bearing on the substantive analysis is tangential, and even distracting. On the other hand, the book contains much interesting information that is competently analyzed. Particularly valuable is the information on land distribution and forms of tenancy in Iran before the land reform. There were 51,300 villages in Iran in the mid-1950s, 76% of all agricultural land was privately owned, state land and endowments (*awqaf*) each constituted a further 10%, and the royal domains accounted for the remaining 4%. A comprehensive agricultural survey in 1960 showed sharecropping to be the most common form of tenure, accounting, with Najmabadi's adjustment, for nearly two-thirds of all agricultural land, as compared with about one-quarter that was "owner-operated." The peasantry was divided into two categories: those with land-use rights (*nasaqdars*), which constituted 41% of the households, and the remaining majority, which was without land-use rights. There was no mechanization to speak of, and credit institutions were completely undeveloped. Money lenders and shopkeepers constituted the major source of credit for most peasants, accounting for over four-fifths of the total amount, as compared with one-tenth from government loans.

As for the land reform itself, Najmabadi's account is commendable in showing that both the discussion of land reform in Iran and the American interest in it predated 1960 and the Kennedy administration. I wish only that she had resisted the typical sociologist's bias of effacing individuals from the historical record and had discussed the motives of the shah and his Minister of Agriculture, Hasan Arsanjani, who was the architect of the land reform.

The most important contribution of the study, however, is Najmabadi's careful and well-documented analysis of the results of the land reform. Except for a short misleading treatment of the statistical (and temporary) effect of the unsuccessful agribusiness schemes as "concentration of ownership" (pp. 101-2), her analysis is superior to that in previously published works. As she rightly emphasizes, the most important result of the land reform was the irreversible change in land tenure and

American Journal of Sociology

rural property relations, which has in turn set in motion fundamental long-term changes in the social structure and in rural economic organization. This is not to say that the extent of land distribution was negligible. The main beneficiaries were peasants with previous land-use rights. Land distribution was thus considerable, much more so than in Egypt (p. 36). But more important than the distribution of land was the drastic change in tenant relations and the termination of sharecropping. Najmabadi states that "owner-operated holdings grew from 33% of the total number and 26% of the total area to 92% and 91% respectively" (p. 107). In this context, the author is careful to distinguish postreform innovations in tenancy arrangements from traditional sharecropping (pp. 113, 164-65).

Other consequences of the land-reform programs that are also well documented include considerable commercialization of agriculture and monetization of rural consumption. Furthermore, the freeing of labor from the land was accelerated as a result of the land reform. Not only did the landless and those peasants with inadequate holdings drift into cities, but paid workers in the agricultural sector itself came to constitute one-fifth of the rural population by the mid-1970s (pp. 130, 136). The establishment of rural cooperatives and development of agricultural credit as a part of the reform program are also surveyed.

In balance, *Land Reform and Social Change in Iran* is the most careful and informative book on the topic to appear to date and as such can be recommended to all students of social change.

Women in the World Economy: An INSTRAW Study. By Susan P. Joeckes. New York: Oxford University Press, 1987. Pp. xii + 161. \$21.00.

Cheryl Johnson-Odim
Loyola University of Chicago

Women in the World Economy is a study commissioned by the United Nations International Research and Training Institute for the Advancement of Women (INSTRAW). It is replete with statistics and "specialist" economic terminology. The book examines women's work in widely disparate geographic regions.

The author, Susan P. Joeckes, raises important general issues about women's work. Among the three most salient are her call for a redefinition of women's productive work to include unpaid labor, a questioning of whether women's work is devalued "only" because of its historical connection to domestic labor/subsistence production, and the crucial linking of women's status not only to gender but also to the economic positions of the particular nations in which women find themselves. Regarding the first, Joeckes points out that unpaid labor is not reflected in labor-force participation statistics, even when it is labor that supplies those things necessary for wage labor to be maintained, that is, the growing and

Book Reviews

processing of family food and the making of family clothing. These facts lead to the underrating of women's economic contributions and have led to their exclusion from development plans.

The author tells us, however, that it may not be solely women's historical role in domestic/subsistence production that occasions the devaluation of their work since women's work that is statistically counted is also systematically found in the lower echelons of the economy. Thus, she says, "There is much to be said for the notion that whatever work women do is devalued" (p. 20).

Joeckes makes a powerful argument that emphasis on "skill" as a determinant of wages, and therefore as an explanation of the depressed wages of women (relative to men) in "unskilled" occupations, is a rationalization. That is, in the lower echelons of the labor force, there is often no real difference in length of training, education, experience, or "know-how" between the jobs men and women perform, though "men's jobs" still pay more money. This is as true of industrialized as of developing nations. Though women as a group make less money than men as a group, the participation of women in the formal wage-labor force does improve their status and material conditions. However, the degree of improvement is heavily dependent on national income. Thus, women's status must be seen as both gender-specific (relative to men in their cohort) and integrated (relative to the positions of both men and women in a particular country and to that country's economic position in the world economy).

One of the author's weaknesses is that she sometimes describes women's position instead of analyzing it. In discussing the transnational (TNC) electronic industry, for instance, Joeckes describes how electronics TNCs are overwhelmingly located in developing countries and display a marked preference for female labor, as reflected by the fact that 80% of their work force is female. Without blinking, she remarks that "Their dexterity may make women especially valuable as employees" (p. 41). When she tells us that frequent turnover rates, low unit costs, and "manageability" are characteristics of this female labor force, we question the degree to which dexterity accounts for the preference of TNCs for female workers. Similarly, Joeckes discusses the fact that the growth of developing countries' manufacturing exports occurs primarily in the clothing industry, which "lies in their specialization and comparative advantage in products made by women" (p. 43). Although the statistics unmistakably show that this female labor is exploited by both indigenous and nonindigenous firms, Joeckes discusses this growth without analyzing its deleterious effects on women.

The book is full of gems of information. For instance, marriage universally gives men claim to higher wages but has the opposite effect on women's wages. This is often attributed to women's expected higher rates of absenteeism after marriage (as well as the belief that men are responsible for the maintenance of families), but in fact women's absenteeism rates are lower than men's. Joeckes writes, "The prevailing wisdom that

American Journal of Sociology

women's absenteeism rates are higher probably derives less from the reality than from the ideological stereotype of the female role, which requires women to give their families first claim on their time" (p. 86).

Joekes makes it clear that, in relation to work, women in developing nations are jeopardized both by the ubiquity of gender-related economic marginalization and by the effects of an international economic order that most often operates to the detriment of their countries' economies. An increasing number of households in the developing world, currently estimated at one-quarter at least, are headed by women. Evidence shows that women's income in low-income families is more often used to improve children's health and nutrition than is men's. These two facts belie the nearly universal belief that those things that improve men's economic status automatically improve the life conditions of women and children.

The book makes an important contribution to studies of women and development by presenting an up-to-date synthesis of a number of such studies of the 1970s and 1980s. Although in the introduction the author warns us that the book is not innovative in its reliance on conventional tools of economic analysis, every chapter, by integrating and centralizing women in even that "conventional" analysis, challenges it.

State and Family in Singapore: Restructuring an Industrial Society. By Janet W. Salaff. Ithaca, N.Y.: Cornell University Press, 1988. Pp. xv + 301. \$37.50.

Donald P. Warwick
Harvard University

By the usual criteria, Singapore's record in economic development has been extraordinary. With the exception of the high-income oil-exporting countries, in 1985 Singapore's per-capita income of \$7,420 (U.S.) was the highest among the 90 developing countries covered by World Bank statistics (*World Development Report 1987* [New York: Oxford University Press, 1987]). Its average annual growth rate of 7.6% for 1965–85 was very high, and its 3.1% rate of inflation for 1980–85 was among the lowest. The government has provided public housing for over 80% of the population and maintains these buildings. The city is attractive, traffic flows smoothly, the air is clean, and crime is low. Some complain that Singapore is sterile in its modernism, but in comparison with Lima, Mexico City, Bombay, or Jakarta, it is an impressive urban setting.

In *State and Family in Singapore*, Janet Salaff is concerned with the effects of economic development and state-sponsored policies on families in Singapore. From 1974 through 1976, she interviewed 100 young married Chinese couples about their experiences with work, marriage, members of their kin group, housing, education, other social services, and family size. In 1981 she reinterviewed 45 of the original couples. The time

interval permitted her to explore the influence on families of Singapore's move in the late 1970s to an export-oriented industrial strategy built around foreign-owned, capital intensive companies.

The book's main contribution is the detail provided on the couples interviewed. The author divides her sample into poor families and secure ones, portrays each in the early and second development stages, and asks how homogeneous Singapore has become as a result of economic development and state policies. Particularly valuable are the vignettes of twelve families, six from each class grouping, at the two points in time. We follow the lives of couples such as the Ngs, a street vendor and homemaker; the Loys, a store clerk and a homemaker; the Gohs, a meter reader and a garment factory worker; and the Chans, a quality-control supervisor and a homemaker. These cases show the changes in jobs and earnings for men, the higher wages earned by women in the new industries, the movement of both poor and secure families to public housing, the increased emphasis on education, the reduction of ties to the extended family, especially among the more affluent, and pressures on all families to hold down their family sizes. Among the poor couples, there is still little communication between husband and wife, while among the secure, marriage is more likely to be seen as a partnership in which spouses exchange confidences. Salaff's interviews and her interpretation of their meanings provide the best account now available of the relationship between economic development and family life in Singapore.

Far less valuable, and often confusing, is the attempt to put the findings in a conceptual framework. Here Salaff passes from the solid ground of family analysis into a haze of political economy. The essential message is that the government of Singapore has used industrial policies, housing, and social services to lure working men and women into its capitalist mode of production. For example, its provision of public housing, widely hailed as one of the country's successes, is presented as a device for integrating the poor into the market and increasing their dependence on the state: "By buying new flats, families become further constrained by the money economy. They must become involved in long-term work goals, mortgage payments, and indebtedness to their pension plans. . . . Continued market activity is necessary for Singapore homeowners, regardless of class position, even in the social service society" (p. 265).

To this reviewer, who has lived in Singapore, Peru, and Mexico, and worked in other poor countries, such conclusions border on the nonsensical. In Toronto, where Salaff lives, the supply of housing with no, or low, down payments and financing through a government-sponsored pension scheme (in Singapore, the Central Provident Fund) would be acclaimed for advancing justice rather than attacked for creating dependence on capitalism. The author herself states (p. 240) that a couple's ability to buy a flat affects their feelings of having "made it." And why is it harmful for homeowners to have long-term work goals and mortgage payments? In North America educational programs try to develop long-

American Journal of Sociology

term goals for the disadvantaged, and the financing of homes by mortgages is considered a social benefit. Many poor Peruvians would gladly sacrifice the "liberty" of their present malnutrition and underemployment for the "proletarianization" in Singapore. The ad-hoc Marxism and the grudging tone in the concluding essay are puzzling in themselves and do not follow from the previous analysis of families.

This book will be of value to sociologists, anthropologists, and others interested in the interactions of social policies, economic change, and family life in developing countries, particularly those in East Asia. The prose is dense and the comparisons of couples sometimes mechanical, but the field research is careful and the portrayals of families convincing. In a second edition the author should replace the final chapter with one that is clearer in conceptualization, less stereotyped in its generalizations, and fairer to what Singapore has accomplished.

Marriage and Inequality in Classless Societies. By Jane Fishburne Collier. Stanford, Calif.: Stanford University Press, 1988. Pp. xv + 290. \$32.50.

Naomi Gerstel
University of Massachusetts at Amherst

For about two decades now, feminist anthropologists have been conducting the kinds of ethnographic research and comparative analyses necessary to challenge the preeminence of "man the hunter" in earlier anthropological thought. In exploring the inequality characteristic of precapitalist and prestate settings, they have insisted on the contribution of "woman the gatherer and toolmaker" while documenting the significance of marriage, family, and kinship in generating gender inequality. Her earlier work made anthropologist Jane Fishburne Collier a central figure in this reconceptualization.

Her new book, *Marriage and Inequality in Classless Societies*, is an ambitious attempt to move beyond this reconceptualization. Even though she remains interested in gender, she does not focus on it. Instead, she embeds her analysis of gender in a broader set of questions about marriage and inequality. On the one hand, this change of focus speaks to both the growth of feminist scholarship, in general, and the depth of Collier's thinking, in particular. On the other, it may leave some feminist scholars uneasy about what has become of women's ties to one another and the strategies that emerged from them in this book that presumes to address inequality in classless societies.

To understand inequality in kin-based, nonstratified societies, Collier analyzes three ideal-typic models of societies and illustrates each model with examples drawn primarily from ethnographies of 19th-century North American Plains Indians: the Comanche (the bride-service model),

Book Reviews

the Cheyenne (the equal bridewealth model), and the Kiowa (the unequal bridewealth model). In all three types of preclass societies, she argues, it is marriage that organizes social inequality and, as a political process, that establishes the kinship bonds that define relations of privilege and obligation. In all three societies, men establish social order (and organize social inequality) by exchanging wives with the support of their kin. While many scholars have recognized that marriage is socially constructed and that its construction is central to gender inequality, few have explored the implications of this understanding as rigorously as Collier.

More specifically, in bride-service societies (which roughly correspond to hunting bands), she shows how marriage requires that husbands offer gifts to and labor for wives' senior kin in return for wives' services (food, shelter, and sexual fidelity). Thus, marriage organizes inequalities among men—between bachelors and grooms and between juniors and seniors—just as wives, not bravery or war honors, are the basis of men's privilege. In the equal bridewealth societies (which roughly correspond to simple horticultural societies), children work for their parents, who arrange marriages for them by exchanging goods directly with the elder kin of a child's future spouse. Elders cannot only give their daughters in marriage; they can also take them away. In this system, grooms appear to need their seniors' help to marry and stay married, and seniors, to be successful, must collect wives, children, and orphans, who provide them with labor. Thus, these equal bridewealth societies, too, have socially created inequalities that are organized by marriage and kinship.

Collier is more tentative about the third type of society—the unequal bridewealth model (roughly equivalent to ranked, but uncentralized, agricultural societies, typically assumed to be in transition). Here, wife takers must give valuables (typically provided by kin) to wife givers. If they cannot do so, they must work, sometimes for life, for the wife givers instead. It is the unequal distribution of labor that explains the ability of high-ranking families to succeed and to give away more than they take. Thus, even in societies with the inheritance of rank, Collier shows how kinship and marriage organize and perpetuate inequality among men.

Each of these three societies, Collier argues, uses a different idiom for understanding the unequal distribution of prestige, power, and privilege. Collier insightfully analyzes the differences among these idioms but suggests that all three serve to mask the social relations they make possible. Thus, she suggests that members of bride-service societies use an idiom of bravery; they discuss men's bravely earning wives without acknowledging the roles wives play in allowing men to appear bravely independent. People in equal bridewealth societies use an idiom of respect; they talk about what seniors do for respectful juniors without acknowledging that juniors produce the goods seniors give as gifts to gain their respect. Those in unequal bridewealth societies use an idiom of rank; they talk about achievements of high-ranking people but ignore the labor performed by low-ranking wife takers that makes the high rank of some wife givers

American Journal of Sociology

possible. What people say, then, reflects not actual relations of power and dependence but rather justifications for behaviors that power relations make possible. Collier argues with remarkable force that such displaced meanings, like the recurring conversations and silences, shape the distribution of resources and aspirations.

Collier also suggests that these discourses misled other ethnographers. While many argued that, for example, men's military skill produced status, Collier shows that men's status was a function of their rights to the products of women, which, when sufficient, allowed men to join raiding parties and obtain military honor. These were mystified by what Collier called "misrecognitions," which served the hegemony of native men and undermined the understanding of Western anthropologists.

As Collier herself recognizes, her typological approach is limited. It obscures ethnographic detail and requires the grouping of societies, some of which have quite different economic and political systems. The centrality of idiom and everyday conversation in her theoretical model seems particularly difficult to substantiate when she must often rely on 19th-century ethnographers' translations (whose theories she often rejects) rather than the discourse of the Plains people themselves. Finally, the book is not easy to read; it is, at times, both turgid and underdeveloped, without adequate illustrative material. Nonetheless, it is sophisticated, insightful, imaginative, and worth the effort.

Feminization of the Labor Force: Paradoxes and Promises. Edited by Jane Jenson, Elisabeth Hagen, and Ceallaigh Reddy. New York: Oxford University Press, 1988. Pp. x + 295. \$39.95 (cloth); \$14.95 (paper).

Rosemary Crompton
University of East Anglia

With its 16 separate articles and 22 contributors, *Feminization of the Labor Force* covers a wide range. The outcome of a conference held at Harvard's Center for European Studies, the book includes papers developing theoretical and empirical material from Canada, Britain, Germany, Italy, the United States, France, and Sweden. The focus, therefore, is limited to the changing pattern of women's employment in Western capitalist industrial countries since the end of World War II. The work is organized into four parts: an introduction, five papers focusing mainly on changes in work and the structure of the labor force, five articles dealing with state policies relating to the position of women and women's employment, and a final section of three papers on the topic of female identity.

On the whole, the editors, Jane Jenson, Elisabeth Hagen, and Ceallaigh Reddy, have done their job well. The general standard of the papers is high and they are sensibly organized—particularly in the second and

Book Reviews

third sections. The introduction and final section are less coherent. For example, it seemed to me that the papers by Veronica Beechey, Anne-Marie Daune-Richard, and Babara Sichtermann were all attempting to grapple with the problem of the boundary between market and domestic labor, which is particularly problematic for women's work. It was not obvious, therefore, why Beechey's work is in the introduction and the other two in the concluding section.

Many of the papers drawing on macrodata (those by Pat Armstrong and Hugh Armstrong, Jane Humphries and Jill Rubery, Elisabeth Vogelheim, and Peter Albin and Eileen Appelbaum) follow in broad outline an argument that is also espoused by Elisabeth Hagen and Jane Jenson in their overview chapter. This asserts that, in the West, the period after World War II was characterized by Keynesian economic policies that encouraged the consolidation of "Fordist" accumulation regimes oriented toward mass, "male," production processes, together with domestic spheres organized around the idea of the "family wage." When women were drafted into the labor force, it was as routine, unskilled workers, or as handmaids in the burgeoning welfare sector. Economic restructuring and the political move to the Right, which have followed the decline of both Fordism and the welfare consensus, have again drawn on the peculiarities of women's labor, in particular its cheapness and flexibility.

This argument does provide a number of useful insights, but it works better for some nation-states than others. The paper by Daniela del Boca, for example, suggests a rather different pattern for Italy, and there are also substantial variations both in the extent of labor-force participation among different countries and in such important features as part-time work. The factors that might explain these variations, are not, however, explored in this book, which, although it contains a wide range of data, is not really a work of comparative analysis. The single paper (by Isabella Bakker) that deploys evidence from more than one country is concerned with descriptions of aggregate-level variations, rather than explanations.

Another problem with the "transition of economic regimes" approach is that it treats women not only as an undifferentiated category but as one that is largely reactive instead of proactive. However, the past two decades have seen a considerable increase in feminist pressure (this book is, in a sense, one manifestation of this) in the countries under discussion here, which is reflected in a number of papers. Those by Jenson (on France) and Mary Ruggie (on Sweden) emphasize the role of the state in bringing about equality of rights (if not always of treatment) for women, whereas those by Ronnie Steinberg, Harold Brackman, Steven Erie and Martin Rein, and Anni Borzeix and Margaret Maruani focus more on the importance of women's own activities in bringing about these changes. They suggest that women, rather than being merely the passive recipients of economic and political forces, have the capacity to affect their own destinies.

It may be suggested that a tension between a relatively passive and a

American Journal of Sociology

more active perspective on the structuring of women's employment is one that underlies many of the papers in this book, although it is not always articulated explicitly. The relationship between the individual and the social structure is, of course, a major issue in social science that is not peculiar to women's employment, but it could be argued that this topic, more than others, forces discussion of the problem. This is because attempts by women to redefine their identities force into the open taken-for-granted assumptions that had assumed the appearance of fixed reference points or institutions. Whether this will result in a revised mix of institutions, as suggested by Gisela Erler in discussing the "new welfare mix" for Germany, or whether we are entering into a period of permanent transience and change with the ending of organized capitalism is a question that the subject of women's employment, and the particular nature of women's work, might have been used to explore.

In summary, this is an extremely interesting and thought-provoking collection. There could have been more genuinely comparative discussion; there could have been a more sustained attempt at theoretical development. However, the scope and richness of the essays that have been gathered together here leave us all in the editors' debt, and the book will no doubt remain a reference and information source for many years.

Deceptive Distinctions: Sex, Gender, and the Social Order. By Cynthia Fuchs Epstein. New Haven, Conn.: Yale University Press, 1988. Pp. xi + 300. \$25.00.

Linda D. Molm
University of Arizona

Both the oldest research on sex and gender and some of the most recent work by such feminist scholars as Gilligan and Chodorow propose that men and women have fundamental differences in character or abilities that contribute to gender inequalities in roles and statuses. These differences are claimed to be rooted in biology, psychoanalytical processes, or socialization. In *Deceptive Distinctions*, Cynthia Fuchs Epstein challenges this position. She reviews the vast social science literature of the past decade that shows that males and females are more similar than different, that most apparent differences are culturally and structurally produced, and that gender inequality is the result of social factors rather than individual differences. She then extends this thesis by exploring how beliefs in gender distinctions create and perpetuate gender inequality and why dichotomous thinking about gender is so pervasive and persistent in the face of evidence to the contrary. Epstein argues that gender distinctions become self-fulfilling prophecies, providing the basis for social arrangements that keep men and women in separate and unequal roles.

Epstein originally began the book, not as an explication of this thesis,

Book Reviews

but as a general assessment of scholarship on gender in the social sciences in the past 10–15 years. It is both, as well as being a history of the changing modes of gender research over the decades. In the book's early chapters, Epstein critiques the methodologies and theoretical perspectives that have produced and maintained the “gender-differences” perspective. She begins with a discussion of methodological biases in gender research and then reviews and critiques biological explanations of gender differences and psychological research on gender differences in cognition, emotions, and personality. These first chapters provide the most direct and effective support for her thesis. Epstein persuasively shows that research espousing gender differences, whether conducted by conservatives or feminists, suffers from methodological flaws and promotes thinking that reinforces gender inequality.

The remainder of the book offers an alternative perspective, a sociological approach to gender that emphasizes cultural and structural determinants of inequality. Epstein shows how both formal and informal social controls constrain the choices and roles of men and women in the workplace, politics, and the family. These chapters provide a useful and fairly comprehensive review of the sociological literature, although they are somewhat less provocative than the earlier chapters. Their content and organization are very similar to those found in several sociological textbooks on gender. Omission of some of the most contemporary issues of gender equality dates the discussion of some topics. For example, the chapter on work makes no mention of comparable worth, the dominant issue of the 1980s, and does not fully discuss either the pervasiveness of sex segregation at every level of work (industry, occupation, organizations, job titles) or its consequences for earnings. The chapter on the family explicitly excludes discussion of the problems of divorce and female-headed households in poverty.

What Epstein does do in these chapters is document the physical and symbolic segregation of men and women in different social spheres. She then explores, in the final chapter, how this segregation helps to maintain beliefs about gender distinctions over time. She focuses particular attention on the question of why women also accept these beliefs even though lower status accompanies them. Epstein argues that as a result of their relegation to routine, low-status jobs in the family and in the workplace, women are kept busy, uninformed, and invisible. Comparisons of men and women are prevented, and the myths that women cannot perform as well as men in areas dominated by men and that the tasks of men are more difficult or valuable are perpetuated. Epstein also suggests that women protect their own sphere of control, the family, by holding that they are more competent than men in managing the home and caring for children—a view that men are quite willing to accept.

The most controversial part of Epstein's thesis, at least for a structural sociologist, is her argument that beliefs in gender distinctions *create* gender inequality. Certainly, these beliefs are used to rationalize and legitimize

American Journal of Sociology

mate inequality, and they help to perpetuate inequality and impede efforts to change it. But do the beliefs of individuals actually cause gender inequality and will changing individual beliefs produce social change? In her conclusion, Epstein appears to suggest exactly that. While readers may disagree with Epstein on this or other specific issues, this is an intelligent and thoughtful book and an important contribution to the growing literature on sex and gender.

A Fallen Angel: The Status Insularity of the Female Alcoholic. By Florence V. Ridlon. Lewisburg, Penn.: Bucknell University Press, 1988. Pp. 180. \$27.50.

Barry Glassner
University of Connecticut

In the American discourse on normal womanhood, there has been no space for heavy drinking. A woman's use of alcohol has signified an incapacity to serve as moral agent and practical sustainer for the family. Florence V. Ridlon's extensive review of the literature in *A Fallen Angel* points out that social scientists, physicians, and the larger public have long envisioned alcohol as a chemical deconstructor of female roles. A woman who drinks heavily is considered prone to promiscuity and therefore unsuitable for marriage, motherhood, or employment in traditional female occupations.

Although a woman might free herself from the confines of traditional roles with the help of alcohol, there has been no favored status opened to her by drink, as is the case for some men. The construction feminine/masculine rests on several other polarities. Among these is hard/soft, and thus the male alcoholic can be glorified for being "the hard-driving, hard-living male," but "there is nothing romantic about the life of the alcoholic woman" (p. 29).

Because there is nowhere in public where women can drink safely, they often drink in silence, alone in their homes. As a consequence of this secrecy, they have been labeled alcoholic less often than men—a fact that Ridlon spends much of her text pondering because she considers it contradictory to the expectation of a prominent theory in the sociology of deviance. On Ridlon's reading of labeling theory, the least powerful are hypothesized to be most prone to labeling. How then, she asks, can women who have less power than men, be less frequently labeled alcoholic?

It seems unfortunate that Ridlon goes to such great lengths to account for this apparent anomaly. Labeling theory, when read from its roots up, as a variety of symbolic interactionism, does not actually contend that particular categories of people are inherently more likely than others to be labeled. Labeling theory understands power as a feature of interactions

Book Reviews

rather than as a property possessed. It is not about causal connections but about social processes, as Howard Becker has clarified on several occasions. For most labeling theorists, it is beside the point to ask whether "the likelihood of being labeled and degree of stigmatization vary together in a positive direction" (p. 98).

Ridlon searches the empirical literature in vain to nail down the conditions under which that relationship holds. Along the way, however, she suggests something important about the societal response to women alcoholics—it has consisted mostly of a deafening silence. Much less research is conducted about women alcoholics, and their condition has been seen as less complex than that of their male counterparts. One result is that fewer resources for treatment have been made available to women.

The silence about women alcoholics extends even to their spouses. While the wives of alcoholics have been described in detail by psychologists, psychiatrists, and social workers (usually as a cause of the husband's pathology), Ridlon could find almost nothing in print about husbands of alcoholics.

Gradually, though, the discourse on drinking is beginning to include women. They are appearing in social science and medical research, and in advertising. Indeed, the liquor industry over the past couple of years has been actively constructing an image of the young professional woman for whom drinking is either a status accoutrement or a role obligation. A recent advertisement shows a man at a bar informing another, "She made Law Review. And she drinks Johnnie Walker." Another ad directs: "Now that you're bringing home the bacon, don't forget the Chivas."

Whether women will lose their status insularity against alcoholism and become equal candidates with men for the label "alcoholic"—and subject to the same explanations of their behavior and similar forms of treatment—remains to be seen.

Learning to Heal: The Development of American Medical Education. By Kenneth M. Ludmerer. New York: Basic Books, 1985. Pp. xv + 346. \$21.95 (cloth); \$12.95 (paper).

Jacquelyn Litt
Allegheny College

In the meticulously researched and richly documented book entitled *Learning to Heal*, Kenneth M. Ludmerer argues that the transformation of medical education that occurred in the United States at the end of the 19th and beginning of the 20th centuries entailed a conceptual revolution in pedagogy, a revolution that was initiated by medical scientists in response to advances in scientific methods and knowledge. These scientists, later called academic physicians, wished to emulate the German system of medical education—its affiliation with universities, use of laboratory

American Journal of Sociology

science, experimental methods, and granting of full-time positions for academicians. Yet they met with stubborn opposition from those mid-19th-century practitioners who, distrusting experimental science and zealously guarding their control over medical education in the proprietary schools, accused academic physicians of espousing a scientific method and a training process that had little relevance to medical practice.

It was not until the late 1890s and early 1900s, with the emergence and apparent successes of modern bacteriology, that academic physicians found increasing acceptance for their principles. A previously skeptical public accepted the new evidence that scientific medicine, while it could not cure every ailment, yielded powers of diagnosis and prognosis that were formerly unthinkable. Moreover, private practitioners were decreasingly hostile to academic physicians and scientific medicine as they recognized the efficacy and promise of this science and began to appreciate a standardized medical education as a strategy to upgrade the status of and decrease the overcrowding in the profession. By 1910, the scientific method provided a powerful common language and collective identity for both the academic physician and the private practitioner. This consolidated professional community now sanctioned widespread educational reform.

The altered understanding of the context of discovery—from observational to experimental, from accepted “truths” to critical inquiry—required changes in the training process. Mid-19th-century medical schools had no university affiliations, required no research, had negligible entrance requirements, guaranteed graduation at the end of an eight-month course of study, taught students by lecture alone, and offered them virtually no contact with patients. By 1910, medical school pedagogy had changed: new tools had to be mastered, experience in the laboratory and, later, in the hospital was necessary, and, most important, students had to adopt an ethos of critical inquiry rather than a passive acceptance of truth handed down by the teacher in the lecture hall. Thus, as early as 1910, these principles of medical education and the role of academic physicians in teaching had been legitimized and their place in medical education was unquestioned.

Ludmerer's data challenge the conventional understanding that the Flexner report of 1910, exposing the scandalous conditions in medical schools, revolutionized medical education. *Pace* previous commentators, the importance of the report lay not in the principles it recommended—laboratory and clinical teaching, affiliations with teaching hospitals, high entrance requirements, and original research—for these had already found legitimacy, largely as a result of academic physicians. Instead, the report “galvaniz[ed]” (p. 167) public sentiment and its demand for licensing; it allowed medical educators greater effectiveness in legitimizing appeals to philanthropists and hastened, but did not initiate, the demise of the proprietary schools through its advocacy of a uniform model of medical education. With medical science as the common language, this

Book Reviews

occupation was able to achieve patronage, state protection (largely due to the efforts of the AMA), and control over education—all prerequisites for the establishment of professional dominance.

Ludmerer demonstrates that science itself does not guarantee this dominance; only the consolidation of occupational interests and public support enabled the occupation to transform its claims to expertise into social and economic privilege. However, when Ludmerer argues that the transformation in medical education was initiated by advances in medical knowledge and the commitment of scientists to advance that knowledge, he fails to examine how social conditions influence why certain scientific choices gain social legitimacy. In this account, the production of knowledge and the scientists' commitment to it are apparently separate from its social uses and implications. Did not scientists' claims to expertise provide not only a collective identity for the occupation and, later, control over the institutions of health care, but also rationales and mechanisms of exclusion from the profession? Moreover, what were the implications of the new scientific method for the public's role in its own health care? In the Progressive Era, science developed as inaccessible, discrete disciplines, too complex for any but the rigorously trained expert to manage. Does this method itself provide the basis for an ideology that disempowers patients and challenges their right to control their bodies?

While Ludmerer's book offers important, new, and useful insights into the process of pedagogic and institutional change in medical education, it does not question the social implications of the scientific method or its claims to objectivity. The reader is grateful to this work not only for the information it contains but also for the research it no doubt will inspire.

The Diffusion of Medical Innovations: An Applied Network Analysis. By Mary L. Fennell and Richard B. Warnecke. New York: Plenum Press, 1988. Pp. xiv + 285. \$34.50.

Joseph Galaskiewicz
University of Minnesota

In this monograph, Mary L. Fennell and Richard B. Warnecke present research findings on the efforts of the National Cancer Institute (NCI) to disseminate state-of-the-art cancer-treatment strategies to hospitals and community practitioners in seven regions of the United States. Several research questions are addressed, but mostly the authors attempt to show how and why strategies were more or less successfully disseminated. Although there is reference to organizational theory and some effort to test hypotheses, the book is highly descriptive, with a strong applied emphasis.

The Diffusion of Medical Innovations is both provocative and frustrating. It clearly shows that models of innovation diffusion that focus on

individual decision making are inadequate for understanding the diffusion of many innovations. Often there is a change agent, which has a mandate to disseminate an innovation and does this through interorganizational channels. In this case the outside actor is the NCI, the innovation is a multidisciplinary treatment strategy for treating head and neck cancers, and the interorganizational channels are the linkages between regional medical research centers and community hospitals. Under these circumstances, models based on individual adaptation are inappropriate since there are dynamics working independently at the national, regional, hospital, and network levels.

Data for the study were taken from records of the NCI, personal interviews with key network participants, questionnaires completed by the chief executive officer of each network hospital, and various secondary sources. The data were collected between 1978 and 1982, and the research was funded by the NCI.

The authors produced three sets of findings. The first describes how the innovation process proceeded differently in each region because its environment and the preexisting interorganizational relations were very different. Apparently this had a telling effect on the way the innovation was ultimately adopted. In leaner environments where diffusion networks were built on the preexisting interpersonal ties between medical centers and hospitals, educational efforts attempted to demonstrate how well the medical centers could treat cancer patients. Resources and decision making were centralized, and linkages between the medical centers and the network hospitals were informal. Not surprising, in these networks patients were referred by network hospitals to be treated at the medical center, and little else was asked of community hospitals or physicians. In richer environments characterized by long-standing interorganizational linkages, educational programs existed at the network hospitals and focused on treatment strategies. Decision making was decentralized, but ties between the member hospitals and the medical centers were highly formalized. In these networks, medical centers funneled resources to network hospitals. They, in turn, treated patients and implemented cancer-control programs.

The second analysis focuses on why some network hospitals participated extensively in program planning and program implementation, while others assumed a more passive role. The researchers found that community hospitals that had a greater number of admissions for head and neck cancers were more likely to be involved in both governance and program activities, and community hospitals were more likely to participate if they were in a smaller network or had the appropriate facilities to implement a cancer-treatment program. These findings suggest that community-hospital participation in networks may depend upon a combination of resources, interest, and network structure.

Finally, they evaluated the effectiveness of the diffusion efforts in each region. From the viewpoint of the NCI, the so-called interorganizational

Book Reviews

networks were the most successful, establishing comprehensive cancer centers and implementing multidisciplinary treatments in their network hospitals. From the viewpoint of the academic medical centers, each of the networks was successful in its own way, as each allowed the medical center to establish its domain over cancer treatment in the region. From the viewpoint of network hospitals and community-based oncologists, the most effective networks were again the interorganizational networks, which distributed network resources to community practitioners or hospitals.

However, looking at data gathered before and after the implementation of the network, the authors found no significant increase in the percentage of larynx and oral cavity cancer patients being treated by multidisciplinary treatments, and there was no significant increase in the percentage of network hospitals with treatment facilities for these cancers; there was, though, a slight increase in the number of otolaryngologists and radiologists per 1,000 population in the six surviving networks. Although the project achieved important organizational goals, it was not clear that the patients were better off.

It is difficult to find fault with a monograph that offers so much detailed information on a complicated topic. The up side of this is a deeper understanding and appreciation of the context in which these networks developed and of the difficulty of studying diffusion when the units of analysis are different. But the reader is frustrated by being overwhelmed with detail. Because it is extremely difficult to operationalize variables like "resource environment," "network structure," and "program implementation," the authors go to excruciating lengths to describe how their variables were measured in each of the seven networks studied. It may be necessary, but it leaves the reader exhausted.

It is even more frustrating that the authors never offer us an alternative model that would explain the relative success or failure of the innovation-diffusion efforts. Instead Fennell and Warnecke adopt a "contingency" approach, arguing that the way innovations diffuse depends upon the environment, the technology, and organizational structures. They never make any firm predictions about how innovations will diffuse but rather attempt to explain the outcome retrospectively, looking at the particular circumstances surrounding the diffusion.

The book is essential reading for researchers in the area of public health, and it does sensitize network researchers to the real-life problems facing someone who seriously wants to model diffusion processes. However, its contribution to network and organizational theory is limited, and I suspect that this will lessen its influence on the literature.

American Journal of Sociology

The System of Professions: An Essay on the Division of Expert Labor. By Andrew Abbott. Chicago: University of Chicago Press, 1988. Pp. xvi + 435. \$49.95 (cloth); \$19.95 (paper).

Paul DiMaggio
Yale University

Andrew Abbott's *The System of Professions* is a signal addition to the literature on its topic, rating a place beside works by Friedson, Larson, and a few others on a narrow shelf of essential texts. The scholarship is impressive: Abbott glides among numerous professions (54, on a partial count) with apparent mastery of historical detail, focusing on the United States but also considering England and France. (The 800-item bibliography is itself a useful resource.) The heart of the contribution, however, lies in five genuinely powerful ideas that complement and challenge previous work on professionals.

First, the appropriate perspective on the development and change of professions is *ecological*. Professions grow when there are niches for them to grow into; they change when other professions threaten their control of particular kinds of work. The history of the professions is the history of recurrent battles over turf.

Second, to understand professions, one must study *jurisdictions*, areas of work over which occupational groups have vied. The key events in professional development are conflicts over jurisdictions. The key environmental changes are those that create new jurisdictions or abolish old ones.

Third, professions constitute a *system*. Struggles over jurisdictions propagate widely: a profession displaced from one jurisdiction may chase a weaker group out of another; a profession that abandons one kind of work to invade a more prestigious or lucrative jurisdiction creates a vacancy into which another profession will move.

Fourth, professional struggle occurs at *three levels*: the workplace, culture and public opinion, and legal and administrative rules. These struggles are loosely coupled: some small professions maintain public hegemony over a type of work but relinquish all but elite practice to the other practitioners. (Thus psychiatry in its prime was unchallenged in what Abbott calls the "personal problems jurisdiction," but most counseling was conducted by social workers, ministers, and lawyers.) The pace of change varies among these three levels: most shifts in jurisdictional control occur first in the workplace, second in public perceptions, and finally, if at all, in the legal system.

Fifth, the most consequential struggles are waged on grounds of competence and theory. Successful professions maintain a "strategic heartland monopoly" over a *core jurisdiction*. Increasing the *abstraction* of professional knowledge enables them to oust competitors from adjacent jurisdictions (as when social problems are subsumed under the medical model), but overabstraction may dilute the cognitive core. Some jurisdic-

Book Reviews

tional conflicts yield decisive outcomes, but pure professional dominance is relatively uncommon. Often contestants divide the work, or winners prefer advisory authority to practice, or the combat results in a standoff, or jurisdiction is partitioned by client type.

These arguments, developed in the volume's first two sections, are illustrated by three historical case studies. The first, a fascinating account of struggles by librarians, computer programmers, operations researchers, and others over the "information" jurisdiction, illustrates the value of juxtaposing professional histories ordinarily considered separately. The second, a comparative study of lawyers in the United States and England, uses court cases on incursions by other professionals to track the nature of professional conflict. The last analyzes the evolution of the personal-problems jurisdiction, with special attention to the decline of the clergy and the rise of psychiatry.

This is a big, contentious book that bursts with arguments and ideas. Naturally, some are less compelling than those I have mentioned. An attempt to analyze all professional work according to an essentially medical tripartite model of diagnosis, inference, and treatment is only partially successful, yielding insights where applied but unlikely to cast much light on the work of most teachers, musicians, or librarians, for example.

The author attacks professionalization models for their ideas of characteristic sequences and their emphases on such structural paraphernalia of professions as associations and graduate programs. Yet his professions pursue professional projects, and the successful ones grab niches in universities; at points, professional associations are used as implicit signs of the existence of distinct professions. To be sure, Abbott demonstrates that professional organization is only the start, not the terminus, of struggle, and he highlights the cognitive, work-based aspects of professional strategy. In doing so, however, he brackets the question of how professionals achieve collective rationality and stints *intraprofessional* conflict. Thus his insights build on and complement (in a most important way) professionalization models, rather than supplant them.

Comparison of this volume's ecological view of professions with the population-ecology's perspective on organizations is instructive. First, the demography of professions plays a key role in the case studies: population models of entries and departures from the ranks of specific professions have great potential for testing several of the book's arguments. Second, Abbott's call to focus on jurisdictions rather than occupations should be taken seriously by population ecologists, who ordinarily focus on organizations rather than niches. Third, the fates of many organizations and the professions that staff them are intertwined; interdependence between the two ecologies deserves close empirical scrutiny.

Few books develop as many compelling and important themes as *The System of Professions*. It will not be the final word on the professions, but it should redirect research attention and keep students of the professions busy for many years.

American Journal of Sociology

Legal Secrets: Equality and Efficiency in the Common Law. By Kim Lane Scheppelle. Chicago: University of Chicago Press, 1988. Pp. xiii + 363. \$54.00 (cloth); \$17.95 (paper).

Mark Cooney
University of Virginia

In *Legal Secrets: Equality and Efficiency in the Common Law* Kim Lane Scheppelle discusses an issue that cuts across many areas of law: the circumstances in which information need not be disclosed. Her central argument is that the large and diverse body of American case law on secrecy can be more coherently explained with a jurisprudential theory she terms "contractarianism" than with the economics-based theory proposed by others.

A secret is "a piece of information that is intentionally withheld by one or more social actor(s) from one or more other social actor(s)" (p. 12). After distinguishing among different types of secrets, Scheppelle proposes that the common law of secrecy can be derived by considering what rules people would consent to in an ideal-choice setting. Using a modified version of the contractarian method employed by Rawls in *A Theory of Justice*, Scheppelle asks what legal rules of secrecy people would create if they knew nothing about whether they themselves were likely to be keepers or targets of secrets. Drawing on experimental evidence, Scheppelle argues that people would want to be protected from catastrophic losses but, given that, would also be willing to take some chances of gaining or losing in their dealings with others. This preference structure would lead rational decision makers to want protection from secrets whose existence is completely unknown to the target of the secret, secrets that the parties have substantially different chances of discovering, secrets acquired in confidential relationships, and secrets held by or against people not capable of making rational choices.

In contrast, Anthony Kronman and Richard Posner advance an economic theory of secrecy. "Economism" holds that the law does and should promote economic efficiency by ensuring that goods wind up at their highest value uses. Kronman argues (see "Mistake, Disclosure, Information and the Law of Contracts," *Journal of Legal Studies* 7 (1978): 1-34) that, in contractual settings, legal protection is granted only to information acquired deliberately and not to information acquired either casually or in the course of another activity. Posner (see *The Economics of Justice* [Cambridge: Harvard University Press, 1981], pp. 231-47) states that personal information can be legally withheld only when it is not discreditable and hence cannot be used to mislead others.

A comparison of these rival theories is the heart of the book. Scheppelle argues that the meaning of a legal rule lies in the factual circumstances to which it has been applied. Reviewing the case law on nondisclosure, fraud, privacy, trade secrets, and caveat emptor, the author finds in each

Book Reviews

instance that the facts highlighted by judges do not relate, as economism suggests, to the manner in which the information is acquired or whether it is discreditable. Rather, the cases evince a persistent concern with equality of access to information, contending that secret holders can withhold only when the other party had an approximately equal chance of obtaining the information.

Lawyers, academic and practicing, will probably find *Legal Secrets* to be an enlightening and useful book. Scheppele brings order to a large body of case law; her writing is clear and concise, and her closely reasoned argument ranges skillfully across a variety of disciplines. The yield for sociologists is slighter. Although the work began as a doctoral dissertation in sociology and although there are a few sociological remarks about secrecy (e.g., its role in both maintaining and undermining stratification, social control, and social structure), it is essentially an exercise in applied jurisprudence. As such, it invites a brief comparison with a sociological treatment of legal matters.

Scheppele seeks to explain the content of legal ideas or rules. Legal sociologists, however, tend to concentrate on legal outcomes (who gets arrested, who sues, who wins). Their empirical research shows that much legal life proceeds largely without reference to ideas about what should happen. Rules are salient only in certain social settings, such as the relationships between organizations and high-status individuals with which the higher courts are concerned (and from which the common law emerges). A concern with the content of rules is therefore not a priority for a discipline that prides itself on its empirical realism. Second, Scheppele explains the rules with the implicit moral philosophy of the judiciary, whereas legal sociologists look primarily to structural variables in accounting for variation in law. Factors such as intimacy, status, reputation, conventionality, and organization are consistently powerful predictors of how cases are handled in the legal system. While the work demonstrating this has been largely ignored by conventional legal thought, it raises some fundamental issues that will eventually have to be addressed publicly (e.g., the attainability of even-handed justice in a heterogeneous society). In short, from the perspective of legal sociology, the rules discussed by Scheppele are akin to the esoteric decrees issued by some imperial monarch: remote, rarified, and frequently irrelevant to the life of the empire.

And yet the monarch's decrees are a type of social behavior, even if they are not as central to legal life as is customarily believed. It is interesting that the primary operational criterion of contractarianism used by Scheppele—equality of information—is also capable of serving in social-structural theory. When legal sociologists eventually turn to the activities of the palace, Scheppele's book may help to explain sociologically the content of its strange utterances.

American Journal of Sociology

Feminism Unmodified: Discourses on Life and Law. By Catharine MacKinnon. Cambridge, Mass.: Harvard University Press, 1987. Pp. 315. \$25.00.

Pauline B. Bart

University of Illinois of Chicago

Feminism Unmodified is to feminism as *The German Ideology* is to Marxism. Using the tools of the sociology of knowledge, Catharine MacKinnon relentlessly uncovers male bias, also known as objectivity, and male perspectives, also known as universality. The book is based on speeches she has given and is thus easier reading than her classic *Signs* articles. The introduction, "The Art of the Impossible," and the afterword are new, and brilliant. On every page are epigrams one wants to print and hang on one's walls, such as, "If it does not track bloody footprints across your desk it is probably not about women" (p. 9), "Women are randomly rewarded and systematically punished for being women" (p. 227), and, lawyers "look at cases the way surfers look at waves" (p. 13). Unlike some other feminist theorists, she is exquisitely aware of the pain of being female under conditions of inequality. Yet she is aware that it is almost impossible to survive if you tell the truth about women's situation.

The book is divided into three sections: "Approaches," which includes a debate with Phyllis Schlafly in which MacKinnon points out that Schlafly, who is qualified to serve in the Republican administration, is being discriminated against because she is a woman. The second section, "Applications," includes her critique of the privacy doctrine on which *Roe v. Wade* is based, her controversial stance that rape is about sex after all because it is about power and violence and power and violence are eroticized in our society, and an evaluation of the first decade of sexual harassment law in court. In the third, "Pornography," she presents and supports her analysis and legal remedy, the MacKinnon-Dworkin anti-pornography civil rights ordinance. Since this book was published, the ordinance was passed in Bellingham, Washington, in a referendum and is currently being tested in the courts.

These are engaged works written because of the pressing nature of women's problems rather than because of academic agendas. In the academic world, there is a convention of claiming to be primarily interested in theoretical and methodological questions, with substantive issues merely convenient data to show which theoretical paradigm can explain more of the variance. She does not play that game. She not only cares about the ERA's being defeated and about rape, abortion for poor women, and prostitution and pornography, but wants to do something about them. She has already done something about sexual harassment, which thanks to her analysis, the Supreme Court unanimously found to be sex discrimination and thus illegal. From her experience in making the system act on sexual harassment, and aware of the limitations of the state

Book Reviews

and of law, she points out law is not everything, but it is not nothing either. She reminds us that the question is not whether one trusts the law to be feminist, for no institution is feminist: "If women are to restrict our demands for change to spheres we can trust, spheres we already control, there will not be any" (p. 228).

The unifying themes of the book are as follows: (1) The social relations of the sexes are organized so men may dominate and women must submit; this relationship is sexual. (2) Gender is not basically a difference but a hierarchy. First, there is gender inequality. Difference comes after and is socially constructed, rather than inherent. (3) In our society, pornography integrates or "actualizes" these two dynamics in life, making inequality sexy and "natural." (4) The mainspring of sexual inequality is misogyny and the mainspring of misogyny is sexual sadism. (I once wrote that analyzing women's conditions without using the concept of misogyny was not worth killing a tree for.) Methodologically her work is based on believing women's accounts of being used and abused, instead of considering such accounts "fantasies" or invoking other psychiatric vocabularies of motivation traditionally used to discredit them.

Sociologist Diana Russell found that only 7.8% of women in the United States (to be precise, in a random sample in San Francisco) have not been sexually assaulted or harassed in their lifetimes. Therefore, she states, "To be about to be raped is to be gender female in the process of going about life as usual" (p. 7). As a woman and as a researcher of sexual assaults, I am aware of how much of my energy goes into arranging my life to be relatively safe. My precautions range from installing a burglar alarm in my apartment to trying to leave my office while it is still light or waiting for the "Red Car" to take me to the parking lot, and using the escort service to walk me to my car in the lot after 7 P.M.—to say nothing of not cutting through alleys, not engaging in friendly banter with male strangers, and all the other ways in which I and other women must constrain our behavior and lower the quality of our lives.

One sociologist criticized this book because MacKinnon has more references to pornography and rape than to orgasm. He is correct. Another sociologist said about MacKinnon, "She's our Martin Luther King." She is right.

The Noble Dream: The Objectivity Question and the American Historical Profession. By Peter Novick. New York: Cambridge University Press, 1988. Pp. xii + 648. \$49.50 (cloth); \$15.95 (paper).

Stephen Turner
University of South Florida

Academic sociology in the United States was born into an already thriving family of disciplines; it was the runt of a litter in which history and

American Journal of Sociology

economics were the older siblings. History was the academic origin of such pioneer sociologists as Albion Small, and Giddings, for most of his career, had "history" in his professorial title. Yet, like rival siblings, the social science disciplines developed by defining themselves in relation to one another. Peter Novick's account of the history of American history in *The Noble Dream* concentrates on one of the defining *differentia*, the historian's concept of objectivity, and traces its shifting course of development from the era of Francis Parkman and historical amateurism to the still-simmering David Abraham affair.

The differences from sociology were quite striking. The historians of the early years of professionalization, especially from 1890 to World War I, were for the most part ultraconservative, and the great historical issue of the day was the healing of the wounds from the Civil War and Reconstruction. Differences, notably sectional differences, were buried under a gentlemanly ethos of comity (quite in contrast to the personal bitterness between the members of the founding generation of American professional sociology) that lent itself to a prosouthern bias. The German academic roots of the founding "professional" historians made the discovery of a long series of continuous institutional developments a standard theme and, under the influence of the problem of national healing, led to the brief dominance of "Anglo-Saxonism," the doctrine of the ancient Teutonic origins of the distinctive political and juridical institutions of the United States and England. "Objectivity" was equated with fidelity to the sources and with an often-expressed horror of "philosophy of history." (The rejection of the philosophy of history was a spur to the creation of a sociological society; "Anglo-Saxonism" in heterodox forms was essential to the background of several early American Sociological Society presidents.)

World War I sharpened "a most genteel insurgency" by the younger generation of James Harvey Robinson, James Shotwell, Carl Becker, and Charles Beard, who challenged the ideal of objectivity (particularly the idea that the historian could, by attending closely to the sources, become free from bias). The excesses of war propaganda, in which historians actively engaged, put paid to this idea and also reminded historians of the importance of history in the schools and the need for a usable past. Thus in the 1920s, as the rest of the social sciences, especially sociology, became objectivist and hyperempiricist, many "Progressive" historians became skeptical and mildly relativistic about historical truth. The professoriat in general was in the doldrums; the relative purchasing power of academic salaries dropped, as did the quality of students and the quality of the positions in which they were placed. Regional academic markets developed, dominated by departments such as Chicago's, and a kind of bland, back-scratching "professionalism" took hold. Yet the 1920s were enlivened by a bitter dispute over the "war-guilt problem," in which Harry Elmer Barnes, wearing his historian's cap, was a leading "revisionist" figure. Comity also broke down over the standard picture of slavery, as

Book Reviews

Progressive historiography assaulted the older prosouthern views of the peculiar institution.

The "relativism" promoted by Beard and Becker defined the dispute over objectivity in the interwar period. Fascism, at the end of the period, discredited it: "war admits of no relativism," as one writer put it in 1940. The Cold War was equally uncongenial to historical relativism, and this, combined with the intervention of foundations and the CIA to prevent a revisionist dispute over World War II and the consequent anathematizing of academic "isolationists," many of whom continued to oppose the expanding American world role, led to a "consensus" historiography of American history dominated by former leftists, such as Richard Hofstader, who had moved Right. The consensus these writers developed on American history was much the same as Parsons's, and shared such idiosyncrasies as his intense antipopulism. It was very much a history congenial to the attitudes of urban northeasters: "asphalt-oriented" as one critic put it (p. 340). The McCarthy era both inspired and reinforced these developments. Major topics such as the Civil War were neglected, and the problem of race was resolved by consensual antiracism. History became intensely professionalized, captive student audiences replaced the larger public as the primary audience, Ph.D. production increased, internal technical questions became dominant, methodological disputes such as the relativism dispute of the 1930s were not pursued, and history as an autonomous, universalistically oriented profession was made secure.

Then the 1960s and the Vietnam War came. Radical historians challenged many of the tenets of consensus historiography, and this challenge combined with the ordinary give-and-take of historical analysis and the availability of new documents to undermine the standard view of Cold War origins and the optimistic version of American history that was key to the older consensus. Out of all this came several kinds of fragmentation. One was the racial and gender particularism of historians who claimed that only blacks could understand the black historical experience, and women who claimed that the history of women should both serve feminism and reflect distinctive nonmale cognitive styles. Another was the result of the rise of history for hire, "public history" paid for or otherwise sponsored and controlled by organizations that wanted their histories written. A third was the particularism that arose between historical paradigms, especially the methodological paradigms of quantification and textualism. A fourth was rampant specialization. The present situation is summed up by the title of one of Novick's last chapters: the center did not hold. The current situation is one that sociologists should find familiar. The machine of text production grinds on, "standards" are more or less upheld but not seriously examined, but the sense of an enterprise with a common purpose has vanished.

This story should be of special interest to historians of sociology, in part because of the peculiar bond of siblinghood. Just as comparisons between national sociological traditions reveal the distinctive, but usually unno-

American Journal of Sociology

ticed, solutions to generic problems that constitute particular sociologies, cross-disciplinary comparisons reveal the distinctive responses to great national events, such as the two world wars and the differences in conception of such common problems as objectivity, which is the theme of Bannister's recent history of the interwar years in American sociology. The text itself is a model history of disciplines, but it is also written in an engaging informal style. Anyone with an interest in history as a discipline will find it an enjoyable read.

The Rhetoric of the Human Sciences: Language and Argument in Scholarship and Public Affairs. Edited by John S. Nelson, Allan Megill, and Donald N. McCloskey. Madison: University of Wisconsin Press, 1988. Pp. vii + 445. \$24.00.

Keith Doubt
Northeast Missouri State University

The Rhetoric of the Human Sciences is an anthology of 22 essays on the uses and problems of rhetoric in all scholarly discourse. The editors, John S. Nelson, Allan Megill, and Donald N. McCloskey, describe their anthology as the demonstration of "an emerging field of interdisciplinary research on rhetoric of inquiry" (p. ix), and they go to great lengths, first, to explain what they mean by rhetoric of inquiry (chap. 1) and, second, to formulate its practical and positive significance in the development of modern scholarship (chaps. 12, 13, 22).

This publication seeks to reinstate Plato's revision of Socrates' critique of rhetoric and cast it in modern pragmatic terms. Whereas in the *Gorgias* Socrates chastises rhetoric as a mere knack that panders to the thoughtless pleasures of the crowd, in the *Phaedrus* he reverses himself and extols rhetoric as a noble and disciplined art without which the scholar would perish. Michael Calvin McGee and John R. Lyne (chap. 21) thoroughly discuss this tension between antirhetoric (Socratic discourse) and sophism (persuasive discourse) and its pressing implications for contemporary scholarship.

Although from different academic fields, the authors share a commitment to the need to resist not only Platonic but also scientific models of authority (foundationalism). Richard Rorty (chap. 3) discusses how pragmatism centers the theorizing and practices of the modern inquiry: "We pragmatists, who wish to reduce objectivity to solidarity, do not require either a metaphysics or an epistemology" (p. 43), and then later, "We should think of 'true' as a word which applies to these beliefs upon which we are able to agree, as roughly synonymous with 'justified'" (p. 45). It is thus intriguing that two mathematicians, Philip J. Davis and Reuben Hersh (chap. 4), are apparently the most Socratic minded. They write, "The philosophy of mathematics is also built up by rhetorical argumenta-

Book Reviews

tion. But the *truth* of mathematics—moving down one level from a discussion of the truth to the truth itself—is considered to be established by means which are the antithesis of rhetoric" (p. 59).

Besides locating the epistemological limit for rhetoric of inquiry in modern scholarship, these essays want to stimulate change. Charles Bazerman (chap. 8) takes aim at the behavioristic orientation of the American Psychological Association *Publication Manual*; we discover that seminal behaviorists such as Wilhelm Wundt and John B. Watson were very forthright in their writing about the difference between reflective thinking and scientific proof and are told that their work would very likely have remained unpublished in these times. Donal E. Carlson (chap. 9) shows that there is already an implicit rhetoric of inquiry in psychology and concludes with this troublesome insight from experimental social psychology: "It is significant that self-awareness has been found to make people's thinking less innovative and more conventional" (p. 157). The sociologist Richard Harvey Brown (chap. 11) challenges the use of reason as a transcendental activity, and his usage is an interesting juxtaposition of ethnomethodology and Marxist critical theory.

While the preceding chapters locate the consequences of rhetoric of inquiry in particular contexts, John S. Nelson (chap. 12) and Allan Megill and Donald N. McCloskey (chap. 13) consider more decisively how and why rhetoric of inquiry develops the quality of scholarly discourse. Then, speaking from a Heideggerian perspective, Gerald L. Bruns (chap. 14) implies that what rhetoric of inquiry is, is the pragmatic path down which the scholar returns to the cave of shadowy discourse. Bruns makes the distinction between analytical knowledge and hermeneutical understanding and indicates that, if what analytical knowledge does is lead scholars out of the cave into the light of knowledge, hermeneutical understanding provides for their necessary return.

What is fascinating about this anthology is the way in which it both mirrors and dissembles Plato's argument for a dialectical rhetoric. In the dramatic conclusion of Plato's *Phaedrus*, Socrates asks, "Shall we leave buried in oblivion men who saw that probability is to be rated higher than truth. . . ?" This publication is now asking the converse question—Shall we leave buried in oblivion men and women who saw that truth is to be rated higher than probability?

American Journal of Sociology

Ritual Healing in Suburban America. By Meredith B. McGuire, with the assistance of Debra Kantor. New Brunswick, N.J.: Rutgers University Press, 1988. Pp. xii + 335. \$39.00 (cloth); \$13.00 (paper).

Animal Liberators: Research and Morality. By Susan Sperling. Berkeley and Los Angeles: University of California Press, 1988. Pp. xiv + 247. \$19.95.

James M. Jasper
New York University

Ritual Healing in Suburban America is a model of clarity, straightforwardly written and organized. Two introductory chapters are followed by four chapters on types of healing groups: Christian groups, traditional metaphysical groups like Unity, groups based on Eastern meditation and human potential, and psychic and occult healing. Each chapter presents the group's characteristic ideals of health and wellness, theories of illness and healing, beliefs about the causes of illness, diagnostic approaches, sources of healing power, healing practices, and measures of therapeutic success and failure. The empirical descriptions of these chapters are then analyzed in four more chapters addressing issues of the role of the healer, the contrast with traditional medical models, the importance of ritual and symbol in social life, and images of the relationship between individual and society.

Meredith B. McGuire's descriptive chapters are, to my taste, less interesting than the analytic ones. They are repetitive, avoid analysis, and do not present much new or surprising information. The presentation is evenhanded, even when dealing with possession by the devil, future lives, and contacting the spirits of the dead. More titillating details might have helped maintain reader interest. The four analytic chapters, on the other hand, raise important sociological questions. Alternative healing practices may provide the meaning, coherence, and sense of control that do in fact bolster one's health, as studies of mainstream medicine have shown. They represent a distrust of the power and authority of medical doctors (although a control group of people not using alternative healing practices was even more negative in its appraisal of physicians). The language, rituals, and symbols of the alternative practices may provide a coherent cosmology that is a comfort in a disenchanted, hyperrationalized society. The healing practices may fit a new, flexible sense of self developing in the new middle class. Unfortunately, these intriguing possibilities are more left as hypotheses than explored with the information gathered on the groups.

My main frustration with *Ritual Healing in Suburban America* is what it does not say about its sample. How typical are these middle-class residents of suburban New Jersey? McGuire is concerned to show that

Book Reviews

her groups do not fit the stereotype of followers of manipulative faith healers and that they are not drug-oriented former members of the 1960s counterculture. But are not these other groups common, too? She wants to ground alternative healing practices in the social and economic situation of the new middle class, but how do we know that other classes do not use them as well? She interviewed a control group of people not involved in alternative healing, but she mentions them very rarely. Nor does she provide data on how extensive alternative healing groups are. In spite of some flaws, however, the book is a good starting point for studies of how meaning—religious and other kinds—is created in everyday life.

Any social movement growing as rapidly as the animal rights movement deserves serious, nonpolemical study, and *Animal Liberators* takes a step in that direction. Although her dissertation adviser was a primatologist criticized by the movement, Susan Sperling manages generally to avoid the worst arrogance of scientists who simply dismiss these activists as kooks. She exhibits the mental grids of the animal rightists: their beliefs about animals, the ways they see animals as human-like, their concerns about technical rationality, and their affinities with the environmental and feminist movements. Her evidence comes from various sources: trends in primatology studies, historical studies of the Victorian antivivisection movement, interviews with Bay Area activists, and the movement's own literature. She convincingly shows that the rise of the movement in the 19th and 20th centuries is due to changes in the moral boundaries that humans envision between themselves and animals. This emphasis on moral and cognitive motivations could have been a useful critique of theories of social movements that focus on group interests, especially material interests, but she ignores that extensive literature.

Unfortunately, *Animal Liberators* has many serious flaws. Its original research seems restricted to interviews with nine Bay Area activists and an interpretive reading of recent primate literature. Yet Sperling generalizes to the animal rights movement as a whole, presumably on the assumption that all activists share one worldview, accessible from any part of the movement. For her the only target of the movement seems scientific research, whereas the movement also addresses substance testing, farming, furs, the extinction of species, and general issues of cruelty. She devoted much of the book to the Victorian antivivisectionists, already the subject of solid research, to which she adds little. At times she seems to set up a comparison between the current and Victorian movements, although she finds no explanatory factors that differ between the two settings. While Sperling details the beliefs of the movement, she offers no clear explanation for the movement's rapid rise. She mentions the trend toward urbanization that has left most of us with only one relationship to animals, that of pets. Yet she says we should not view the activists as predominantly women with unusual bonds to their pets. How then should we view the activists? How do we get from the broad social trends she describes to an account of the movement?

American Journal of Sociology

Ritual Healing and *Animal Liberators* are linked in more than just the imagination of an *AJS* editor. Educated, middle-class Americans are dissatisfied with the instrumental rationality they encounter in their daily lives. Instead of turning to traditional religions for an alternative, they often pick up ideas, revived in the late 1960s by the environmental movement, that humans are not the measure of all things, that there are forces beyond us we can only tap into, that nature has a moral value all its own, that reality is not exhausted by our ability to control it. Animal rights and alternative healing may be surface phenomena of major changes in contemporary belief systems, to which sociologists should pay attention.

Making History: The American Left and the American Mind. By Richard Flacks. New York: Columbia University Press, 1988. Pp. xii + 313. \$35.00.

Richard A. Dello Buono
Rosary College

Richard Flacks's central motive for writing *Making History* is to explain why "people persist in preoccupation with everyday life even though our culture values public participation and social responsibility, and the left offers them a vision in which they can shape their own future" (p. 8). The key, he suggests, is to be found in the fundamental political schism that separates our private and public lives. Americans simply lack the motivation to "make history" given the ample private "space" that exists for comfortable everyday living. Accommodation to powerful elites prevails because we are aware of the personal sacrifices necessary for intervening in the making of history. Flacks argues that nothing short of the democratization of the entire American political economy can transcend this fundamental schism.

According to Flacks, the vast majority of Americans in the post-World War II period found satisfaction in the nonworking, "leisure" portion of their lives. In contrast to the "cultural critics" who argue that commodified leisure activities are shallow and meaningless, Flacks maintains that such activities provide a cornerstone of private stability and complacency for the average American. The vast array of choices within these commodified leisure activities became fused with the ideological concept of "liberty," thereby making the intensive living of one's private life equivalent to the active practice of American freedom.

Flacks asserts that the dialectics of this quietist period in our history comes down to the following modus operandi, which the masses offer to history-making elites: "We will leave you alone if you leave us alone" (p. 57). Assuming that the elites can "deliver the goods," the majority of Americans are content to struggle only to defend their liberty, that is, to defend against any encroachments on their private spheres. How then can history be made? According to Flacks, it is when the patterns of

Book Reviews

everyday life are threatened that people begin to make history through collective resistance.

Resistance movements are *democratic*, Flacks argues, because they draw the powerless into collective political action. The New Left was "new" because it rejected the Leninist model, the extreme case of an organization transformed into an end in itself (p. 213). Consequently, the Student Nonviolent Coordinating Committee (SNCC) and the Students for a Democratic Society (SDS) both "learned a great deal from the flaws of their predecessors" (p. 204). As a national organization open to all those who accepted its general value framework, the SDS (1962-69) stands as Flacks's model of democratic organization for the Left.

It is suggested that one way for the Left to avoid repeating the self-defeating mistake of elitist organization building is to recognize seriously the ways in which history can be made in daily life (p. 242). Apart from the fact that this directly challenges his own distinction about making history as "the opposite of making our daily lives," Flacks essentially argues for the Left to articulate itself as a cultural presence rather than a purely political one. For example, politically conscious child rearing is identified as a matter of critical importance.

Flacks's diatribe on the failure of vanguard parties is unconvincing and predictably leads him to brand all notions of armed struggle as "terrorism." This retrospectively projects an unwarranted image on genuine peoples organizations, such as the Black Panther Party, and has unfortunate implications for the various support networks of national liberation struggles. For Flacks, the democratic alternative to immoral and impractical violence is pacifism. Left activists must understand that "their mission is to serve as exemplars of moral being" (p. 275).

The bulk of Flacks's argument consists of a subjectivistic hodgepodge of selective caricatures embodying his fatally flawed, social psychological framework of analysis. The reader is subjected to a continuous series of carefully orchestrated, anecdotal reflections on the Old and New Left, all punctuated by numerous recitations of ahistorical sociological truisms. Meanwhile, virtually no structural analysis of the coalitions that actually formed the New Left is offered.

The singularly enticing thread that runs throughout the first five chapters is a promise that the overall analysis will lead to new insights into the prospects for revitalization of the Left. Yet there is no attempt to analyze the social conditions that would promote such a development. Would, for example, declining middle-class standards constitute the sort of political watershed envisioned in this larger framework?

Most notably absent from this work is any serious consideration of the role played by American Left organizations in popular movements of the 1980s. In the end, we are simply presented with the judgment that the ongoing national effort to build a left-wing coalition in the Democratic party is the only viable formulation currently available to the Left (p. 276). We are left wondering how many SDS members in 1968 would have agreed.

NEW SOCIOLOGY

Freedom and Taboo

Pornography and the Politics of a Self Divided

Richard S. Randall

Randall provides a new look at pornography both as a part of the human psyche and as a public policy issue. He contends that pornography is a universal phenomenon, rooted in the human condition itself, rendering the human being, alone among species, the "pornographic animal." \$29.95

Beneath the Miracle

Labor Subordination in the New Asian Industrialism

Frederic C. Deyo

Deyo examines the remarkable economic transformation of Asia, which has been based in large measure on the production of manufactured goods for export. Because their competitive edge is rooted in the mobilization of a low-cost, disciplined, and productive workforce, Deyo analyzes the management of this workforce. \$30.00

The Social Importance of Self-Esteem

Andrew Mecca, Neil J. Smelser, and John Vasconcellos, eds.

This volume draws on research in the social and behavioral sciences to explore the connections between self-esteem and social responsibility. It poses a challenging and provocative counter-emphasis to the assumption that social institutions are the primary determinants of individual welfare. \$9.95 paper, \$40.00 cloth

The Power of Collective Purse Strings

The Effect of Bank Hegemony on Corporations and the State

Davita Silfen Glasberg

Davita examines the power that financial institutions and the state exert over the allocation of capital. She explores how financial institutions play an important role in the way corporations deal with a variety of crises. \$30.00

Literature, Politics, and Culture in Postwar Britain

Alan Sinfield

"A major work of cultural criticism in the British Marxist tradition. It makes a significant 'intervention' in the very thing Sinfield is talking about, the social construction of literary and political values. . . . Controversial . . . brilliant."—Patrick Brantlinger, author of *Rule of Darkness*

\$45.00 cloth, \$14.95 paper

Into One's Own

From Youth to Adulthood in the United States, 1920-1975

John Modell

"Will stand for many years as the definitive American story of growing up and entering adult life since the First World War. . . . We can move beyond 'then-now' comparisons to a genuine understanding of the connecting pathways across the twentieth century. A splendid achievement!"—Glen H. Elder, Jr., University of North Carolina at Chapel Hill \$40.00

Strategies for Learning

Small-Group Activities in American, Japanese, and Swedish Industry

Robert E. Cole

Cole examines small-group activities in the United States, Japan, and Sweden during the past 25 years. The striking national differences he finds reflect the varied historical experience of each country and, perhaps more important for the future, have serious competitive implications. \$29.95

Fleeing the Iron Cage

Culture, Politics, and Modernity in the Thought of Max Weber

Lawrence A. Scaff

"Enriches the discourse about Weber and leads us back to what I believe to be a more authentic approach to his magnificent achievements."

—James Sheehan, Stanford University
\$35.00